

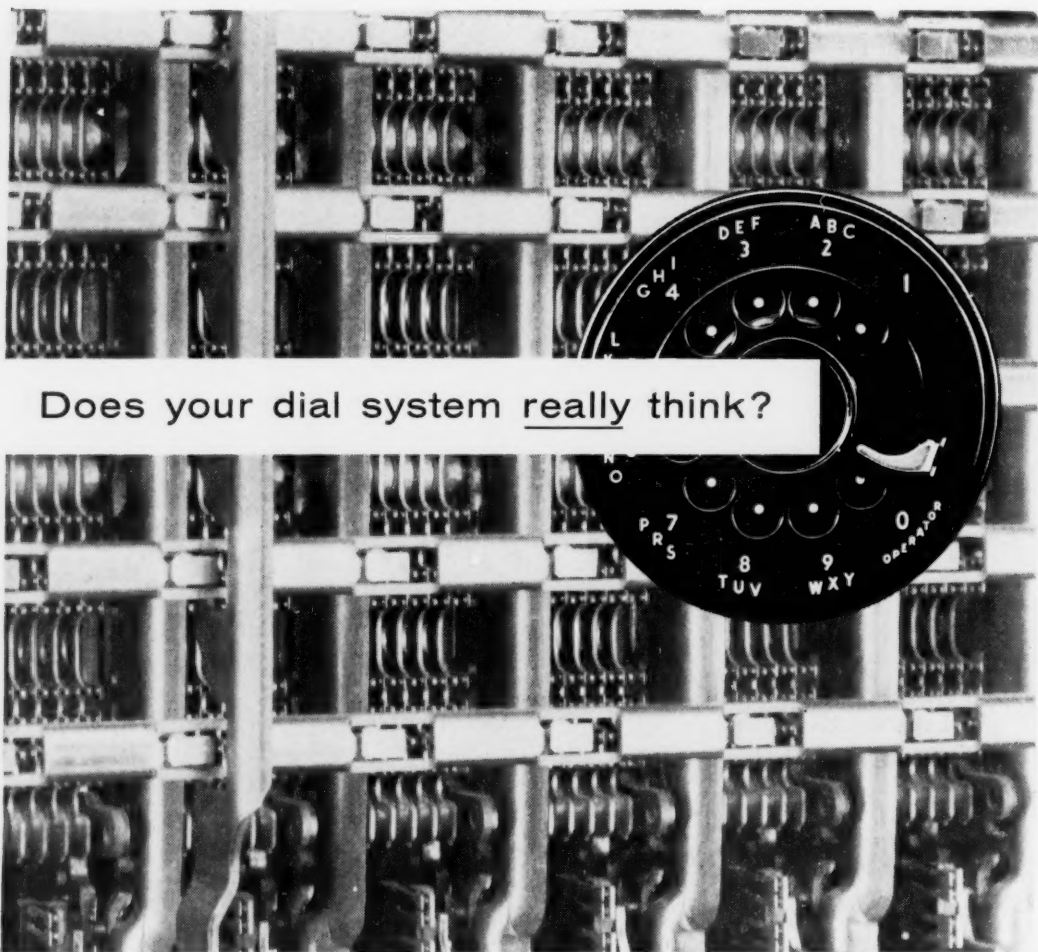
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[Courtesy Emil Seletz, see page 85]

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THE SCIENTIFIC MONTHLY, established in 1872 as *The Popular Science Monthly*, has been an official publication of the American Association for the Advancement of Science since 1915. It is published for the Association at Business Press, Inc., Lancaster, Pa.

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All correspondence should be addressed to THE SCIENTIFIC MONTHLY, 1515 Massachusetts Ave., N.W., Washington 5, D. C. Cable address: Advancesci, Washington.

Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of

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Annual subscriptions: \$7.50, domestic and foreign. Single copies, 75¢. Special rates to members of the AAAS.

THE SCIENTIFIC MONTHLY is indexed in the *Reader's Guide to Periodical Literature*.

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Science and Technology

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Weathercaster

The Guest Weathercaster is a booklet-instrument designed for making local weather forecasts. Predictions for areas within 30 to 50 mi and times of 12 to 24 hr are made by setting each of four concentric circular dials on the front cover of the booklet in accordance with present observations, and then referring to tables for the forecast. The dials are called the wind, barometer, barometer change, and present weather dials. After each of these dials has been aligned on the index, a letter-and-number code can be read. This code is then applied to the code tables in the booklet. The code tables in turn produce a weather prediction key by means of which 1 of 21 different weather conditions, 1 of 8 wind velocities (modified Beaufort scale), and 1 of 8 wind directions are predicted. The instructions are clear and complete. (Guest Products Corp., Dept. SM, 381 4th Ave., New York 16.)

Miter Doweling Jig

A new doweling jig simplifies the problem of locating and drilling dowel holes for mitered joints in wood, plastic, and light metal stock from $\frac{3}{8}$ to $1\frac{3}{4}$ in. in width. The jig is made of electric-welded heavy-gage steel and is fitted with a centering gage and an adjustable drilling block that will accommodate up to four holes per miter with $\frac{1}{4}$ in. drills. (Gunver Mfg. Co., Dept. SM, Hartford Rd., Manchester, Conn.)

Pocket Comparator

"Pee Gee" pocket comparator, designed for measuring small dimensions, has a six-power lens. The body is made of clear molded Lucite, and the adjustable lens barrel and reticle retainer ring are made of machined aluminum. Several reticles are available: "scales" for measurements from 0.0025 to 0.5 in. in steps of 0.0025 in. and 0 to 10 mm in steps of 0.2 mm; "radii," $1/16$ to $3/8$ in. in steps of $1/32$ in.; "angles," 0° to 90° in 5° increments; "threads," threads per inch; thickness," 0.002 to 0.016 in. in steps of 0.001 in.; and "diameters," 0.005 to 0.1 in. and $1/64$ to $1/16$ in. in steps of $1/64$ in. (National Tool Co., Dept. SM, 11200 Madison Ave., Cleveland 2, Ohio.)

Air Filter

Dustrap filters are designed to clean the hot air in hot-air heating systems. They can be installed either in the furnace heat pipes or immediately under floor registers. A soft metal retaining band is supplied to hold the filter in place and seal the opening when the filter is installed in heat pipes. The dust is caught by a solution on the mesh, and the filter can be washed for further use. (Upstaters, Dept. OO-SM, Penfield, N.Y.)

Water Demineralizer

A new demineralizer with a 4-gal storage tank operates automatically. Water flows through a float valve into the top of a two-section resin column 24 in. long. After the resin in the upper section is exhausted, it is replaced and the column is inverted; thus only half the resin has to be replaced at one time and waste is reduced. A flow-control regulator assures that water flows through the resin column at an optimum rate. (Scientific Equipment Corp., Dept. SM, 5438 Lowell Ave., Indianapolis 19, Ind.)

Molded Drawers

Drawers molded of Bakelite phenolic resin can be used in kitchen cabinets, chests, desks, and other articles of furniture. The drawers have smooth splinter-free surfaces that do not require painting or finishing, and they are not affected by moisture, heat, or most chemicals. A descriptive brochure is available. (Bakelite Co., News Bureau, Dept. SM, 30 E. 42 St., New York 17.)

Ringstand Microadjustor

Laboratory workers can use a new ringstand microadjustor (Fig. 1) designed by Fred E. D'Amour of the University of Denver to avoid the difficulty encountered in raising or lowering the clamp holding a piece of apparatus when it is clamped directly to the ringstand. The apparatus is clamped to the sleeve of the adjustor in approximately the desired position; it may then be raised or lowered very precisely by means of a knurled thumbscrew. The sleeve is tongued to a slot in the vertical bar so that it cannot rotate. (Phipps and Bird, Inc., Dept. SM, Richmond, Va.)

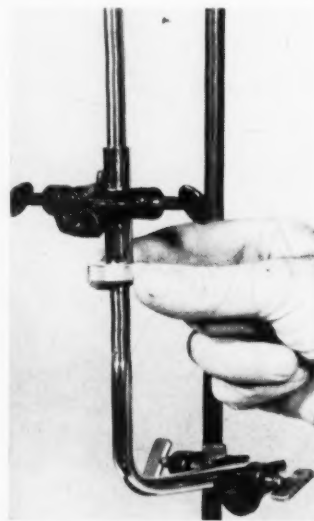


Fig. 1.

Micrometer

A new indicating micrometer (Fig. 2) provides visual indication of measuring pressure. In addition, out-of-roundness and taper can be determined by rotating or sliding the work between the anvils while observing the indicator. The measuring capacity is 1 in. and the indicator reading is in 0.0001 in. Resetting to zero requires 5 sec., and the release button for the movable anvil is on the right-hand side. (George Scherr Co., Inc., Dept. SM, 200 Lafayette St., New York 12.)

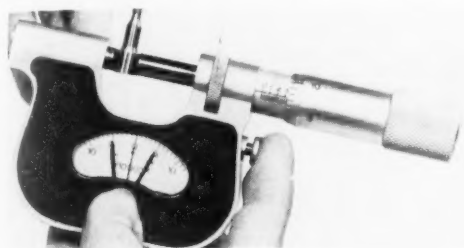


Fig. 2.

Rear-View Mirror

A new automobile mirror makes use of a "beam-splitting" technique and a small three-way "gear shift" to reduce glare in both day and night driving. The front surface of the glass, the beam splitter, is coated with three microscopic layers. These layers reflect some colors toward the driver and allow others to pass through to the rear surface before they are reflected. The gear shift changes the mirror angle and determines whether the driver sees a reflection from the front surface or a reflection from the rear surface after the light has been filtered by the front surface. (Liberty Mirror Div., Libby-Owens-Ford Glass Co., Dept. SM, Brackenridge, Pa.)

Electric Paint Remover

A new electric paint remover utilizes a metal-enclosed heating element that heats in 90 sec. It incorporates a front-end scraping blade and operates from standard household electric outlets. (Shepherd Machine & Die Co., Dept. SM, 15215 Chatfield Ave., Cleveland, Ohio.)

Antirust Compound

End-O-Rust, a zinc, resin-vehicle coating that is applied by brush, bonds to any iron or steel surface that is dry and free from grease, loose scale, tar, and bituminous paint. (Industrial Craftsmen, Inc., Dept. SM, 145 High St., Boston 10, Mass.)

Slide Rule Pencil

A sliding-bar slide rule has been built into a new pocket-sized mechanical pencil. The pencil comes in two models, one with a six-scale slide rule and one writing color, the other with an eight-scale rule and two writing colors. (Device Development Co., Dept. SM, 226 W. 4 St., New York 14.)

Refrigerator Dispenser

The Jarco refrigerator dispenser consists of a rubber bulb, a spout, aluminum tube, 1-gal jar, and fastening device. A quick push on the top of the rubber bulb dispenses approximately 7 oz of whatever liquid is stored in the jar. (Jasper Rubber Products, Inc., Dept. SM, Box 168, Jasper, Ind.)

Telescope

Designed for the beginner in amateur astronomy, the Palomar, Jr., is a 4¼-in. reflecting telescope with rack and pinion focusing and equatorial mounting. The mirror mount, which is adjustable and removable, is housed in an aluminum tube. The focal length of the mirror is 45 in., and the instrument is tripod-supported. (Edmund Scientific Corp., Dept. SM, Barrington 3, N.J.)

Burette Support

A new white porcelain support stand has been designed especially for laboratory workers doing one-burette titrations with difficult-to-see endpoints. The dimensions of the base are 9 by 9 in. The 7-in. distance between the aluminum support rod and the front of the base allows room for placing vessels on the base. (Fisher Scientific Co., Dept. SM, 717 Forbes St., Pittsburgh 19, Pa.)

Exposure Meter for Photomicrography

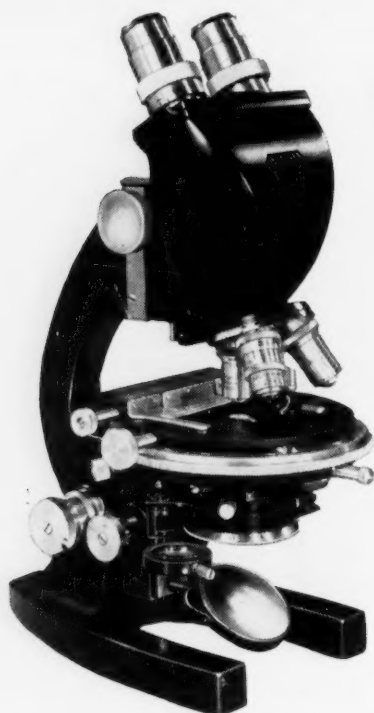
Microphot is a new exposure meter designed for use with photomicrographic equipment. The 2- by 2-in. barrier-layer type photocell is completely sealed. The instrument, which can be used either with a pointer galvanometer or an external mirror galvanometer, has four sensitivity ranges, two using the full cell and two using a reduced surface. At maximum sensitivity one scale division corresponds to 0.004 ft.-ca. (Brinkmann Instruments, Inc., Dept. SM, 378-380 Great Neck Rd., Great Neck, N.Y.)

Metalworking Lathe

A cabinet model 11-in. metalworking lathe with 24-in. center-to-center capacity and 1-in. collet capacity has been announced by Delta. The diameter of the hole through the spindle is 1⅜ in. The machine has a back gear shift lever located on the front and continuously variable speed from 44 to 1550 rev/min. (Rockwell News Bureau, Dept. SM, 400 N. Lexington Ave., Pittsburgh 8, Pa.)

Camp Stove

A portable camp stove that utilizes a disposable liquefied petroleum fuel tank has been announced. The unit weighs 6 lb and is packed in a 5½- by 5½- by 11-in. metal carrying case that can be used as a windshield. The fuel tank has a pressure relief valve and an automatic self-sealing center inlet. No pumping, priming, or warmup is required. (Turner Brass Works, Dept. SM, Sycamore, Ill.)



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THE SCIENTIFIC MONTHLY

FEBRUARY 1955

The United Nations in Perspective

JOHN B. WHITTON, CHARLES G. FENWICK, BENJAMIN
GERIG, ROY BLOUGH

Dr. Whitton, who is associate professor of politics at Princeton University, has been a close student of international law since 1920 and has taught extensively in this country and in Europe. Dr. Fenwick taught at Bryn Mawr College from 1914 to 1945 and is now director of the department of international law of the Pan American Union and president of the American Society of International Law. He is the author of standard works in the field of international law. Dr. Gerig, who has long been an authority on the mandates system and trusteeship, is director of the Office of Dependent Area Affairs of the U.S. Department of State and deputy representative of the United States to the Trusteeship Council of the United Nations. Following a period of teaching at the University of Chicago, Dr. Blough served as a member of the President's Council of Economic Advisers from 1950-1952. He is now principal director of the Department of Economic Affairs of the United Nations. The viewpoints in this article are those of the authors and are not necessarily represented as official. This article is based on papers presented at the annual fall sessions of the National Academy of Economics and Political Science; Section K, AAAS; and the National Social Science Honor Society. Pi Gamma Mu; at the auditorium of the U.S. Department of State, Washington, D.C., 20 and 21 Oct. 1954.

Control of the Veto in the United Nations Charter

THE problem of the veto is the *point nevralgique*, the crucial problem of international organization. Nothing has given students of world peace more headaches or caused more ink to flow than this perplexing question. How a single negative vote may threaten a great international congress with disaster was shown recently with dramatic force when Mendès-France was prepared to invalidate the London Conference just when its success seemed assured. The veto was likewise a thorn in the side of the League of Nations. In 1926 Brazil and Spain by their stubborn opposition for 6 months paralyzed the efforts of that body to bring Germany in as a permanent

member (1). From the San Francisco Conference to the present, the use of the veto in the Security Council has been the cause of deep concern, the center of continuous controversy. Already employed no less than 61 times, all but twice by the Soviet Union, it has materially reduced the activity of that body, as crucial problems are dealt with increasingly by outside agencies, notably the General Assembly. The result has been embittered relations among the powers, mounting dissension, and the advocacy, by sincere friends of peace, of extreme remedies through law. Some, antagonized by the U.S.S.R.'s tactics, would even go so far as to expel the U.S.S.R. from the United Nations, although how this could be accomplished

over the U.S.S.R. veto has not been explained. Others are prepared to establish immediately some form of international government or world federation of nations. Even those who still believe that our best chance is to continue to work for peace through the United Nations are making an agonizing reappraisal of the entire system of voting in the Security Council. This movement has gained ground because of the scheduled conference on charter revision, now the subject of intensive study by both government and unofficial agencies.

The problem of the veto, however, is not new. On the contrary, it has always plagued every movement to set up over individual states some degree of collective control. It is the problem of conciliating the demands of individual sovereignty with the exigencies of collective authority. This basic difficulty is bound to arise whenever an attempt is made to establish some superior agency with authority to tell the individual subject of law what it may or may not do, whether the group consists of individuals or of six or 60 states. Long ago, within the state, the individual was forced to give up his freedom of action. This was accomplished in the general interest as a matter of sheer necessity. Otherwise there would be anarchy, *Ubi societas ibi jus*. "Sovereignty," to use that difficult word, was first given up by the individual subject of law in the tribe, the city, the country, and the nation-state. The last and final step, the establishment of the rule of law in the international society, is yet to be accomplished.

That it has taken so long to accomplish this final task of international law is the result of many factors, which cannot be analyzed here. But, if attention is fixed on the major undertaking, the attempt through international organization to establish a system of collective security, certain basic difficulties may be perceived, still unsolved, that underlie the problem of the veto. At Dumbarton Oaks it was believed necessary to endow the great powers with special responsibilities and to grant them exceptional powers. A voting formula was adopted whereby the Security Council could take no action without seven affirmative votes, and the permanent members were each granted a right of veto with respect to all important matters. This formula protected the great powers, but it failed to assure for the world at large a reliable system of collective security. For in any emergency a permanent member by its sole veto could not only avoid any obligation of acting itself but could at times prevent anyone else from acting. As the matter evolved, all decisions made by the Security Council require a majority of seven votes. If the matter concerned is one of substance, and not of

procedure, the majority of seven must include the affirmative votes of all five permanent members. Here, then, is the rule of qualified majority, otherwise known as the veto.

There are two important exceptions stipulated in the now-famous Article 27, and there are other exceptions set forth in the Four-Power Statement. Under Article 27, it is stipulated that for decisions taken under Chapter VI, which is concerned with the pacific settlement of international disputes, no party to the dispute is permitted to vote. But, of course, any recommendation under Chapter VI may be vetoed by a permanent member other than a party. And if in the evolution of the dispute it becomes necessary to use force against one of the parties to reestablish peace, the veto applies, even where the aggressor happens to be a great power. The latter may veto action against itself. Another exception is that a party to a dispute may not vote when, under paragraph 3 of Article 52, the Security Council acts to

... encourage the development of pacific settlement of local disputes through such regional arrangements or by such regional agencies either on the initiative of the states concerned or by reference from the Security Council.

In the Four-Power Statement, which has been a center of much controversy, no individual member of the Council alone could prevent consideration and discussion by the Council of a dispute or situation brought to its attention. It was also stated that the parties to a dispute could not be prevented by veto from being heard by the Council. The Statement also included the famous "chain of events" theory, of very doubtful validity. Any move to order an investigation, or to take any other step beyond mere discussion, could well lead, it was alleged, to a situation in which the Council would be called on to order measures of enforcement. Here, unless unanimity was assured at the very start, by permitting the resort to the veto *ab initio*, the result might well be a disruption of the unity of the powers, the basic assumption underlying the entire Charter. Another important paragraph in the Statement declared that the preliminary question of whether a given matter was one of substance or procedure was itself a matter of substance. In other words, a decision on this preliminary matter would require a vote of seven members, including that of all the great powers, thus opening the way to the exercise of what has been termed the double veto. Finally, the Statement contained the following which, in the light of what has taken place since, has today a very hollow sound indeed:

It is not to be assumed . . . that the permanent members, any more than the non-permanent members, would use their 'veto' power willfully to obstruct the operation of the Council.

This part of the Statement has been cited since 1945 more often than any other. The disputed voting formula was finally incorporated as Article 27 of the Charter, but many delegates voted for it only in the hope that the veto would not be abused or, if it were abused, that the matter could be remedied through a revision of the Charter.

It was not surprising to find all the great powers at San Francisco justifying the veto, by any permanent member of a motion declaring it to be an aggressor, on the ground that police action against a great power is not feasible in any event. But this assumes too much; it apparently rests on the conclusion that the only possible sanction against an aggressor is the employment of force. This is a serious *non sequitur*. As is specified clearly in Article 41, the Security Council has complete discretion on what measures to employ against a state that resorts illegally to force or the threat of force. Consequently, the following system for voting in this matter would greatly strengthen the United Nations organization. First, deprive the alleged aggressor of a vote when its own acts are being considered, this in accordance with the age-old maxim found in all civilized systems of law, *nemo debet iudex in propria sua causa* (no one shall be judge in his own cause). Second, leave it to the discretion of the Council to decide in the circumstances what sanctions to apply. If the great power were just too strong to be opposed by force, perhaps the international organization would have to be content with mild sanctions, such as a mere withdrawal of diplomatic agents. Even if it only voted to censure the aggressor, the international organization would nevertheless conserve its moral position. But, under the Charter, the Security Council cannot even *censure* a permanent member, unless the erring member is thoughtful enough to grant its consent!

It was with great misgivings that the vast majority of the delegates at San Francisco accepted the Yalta formula governing the vote in the Security Council. And, ever since, there has been a continuous movement to attenuate the rigors of the veto. The matter has been given extensive study in the General Assembly and in the Interim Assembly, and many admonitions have been sent down from these bodies to the Council, calling on the permanent members to use the veto with discretion (2). The United States Government for its part has announced that it favors the abolition of the veto, both on the admission of members and

on action under Chapter VI for the pacific settlement of disputes—but to no avail. But fortunately, in actual practice, the Security Council has interpreted Article 27 in a manner that, to a considerable degree, has attenuated its original severity.

In the first place, in exceptional circumstances the resolution of the Council, although vetoed, has nevertheless been implemented by the parties concerned. A second development is the practice of not counting abstention from voting as a negative vote. It often happens that a member of the Council, although actually disapproving of a given resolution, does not wish to frustrate the action desired by the majority. At other times, it may favor the motion but for political reasons does not wish to vote in the affirmative. The nice solution in such a situation is to abstain. It is very doubtful whether this interpretation is in accord with the intentions of the founders of the Charter. The versions of Article 27 other than the English text provide that in decisions of the Security Council on matters of substance, *all* the permanent members must vote affirmatively. But this practice of abstention has been followed numerous times, and no one has officially contested it.

Another development, and the one most mooted, since it occurred at the crucial moment of Council action on the initial aggression in Korea, concerns the interpretation of *absence*. In that case it was ruled that the absence of the permanent member, Soviet Russia, would be considered to have the same effect as abstention; that is, it would not count as a veto. When the Security Council, in June 1950, voted to recommend the use of sanctions against North Korea, the Russian delegate was voluntarily absent, having walked out several months previously in protest against the continuous presence in the Council of the delegate from Nationalist China. This was indeed a fortunate circumstance—along with the presence nearby of American troops, and the fact that a United Nations Commission happened to be on the spot to report immediately the actual facts of North Korean aggression. An even more fortunate circumstance was the fact that the defense of South Korea was a matter of vital national interest to the United States. In the absence of the Soviet delegate, the Council was enabled to act immediately, without fear of the inevitable U.S.S.R. opposition in that body. But ever since it has been a moot point whether this action by the Council was in accordance with a correct interpretation of the Charter. In the particular case of Korea, the Council implicitly, by acting without the Soviet delegate, ruled its own action to be legal, thus following the accepted practice among United Na-

tions organs, each one deciding on its own competence without appeal to a higher authority. But whether this interpretation of the Charter was correct has never been decided.

There is still one more development whereby in actual practice the United Nations has shown an ability to deal effectively with threats to the peace, despite the use in the Security Council of the big-power veto. This is through a resort to the General Assembly. At times, for fear of a Soviet veto in the Council, the matter is referred directly to the General Assembly. Thus, the Council is bypassed, as it was in the matter of the independence of Korea and the Chinese complaint against Soviet intervention in China. In other cases, after veto, the matter is taken up by the Assembly. In this body, a mere two-thirds majority is required in order to pass an important resolution, so there is no possibility of a veto. Legally, of course, the Security Council may not directly refer a matter vetoed by a permanent member to the General Assembly, unless a qualified majority is obtained. But the matter need not end there, for a motion to take a matter off the agenda may be voted by any seven members. From this moment, the General Assembly may be seized of the question. And after deliberating, the Assembly may recommend appropriate action, even to the extent of proposing that the members apply measures of force to restore peace. It is true that under Article 11, paragraph 2, any matter upon which "action" is required is supposed to be referred to the Security Council. But if such "action," because of the veto, is impossible, a logical interpretation of the Charter should justify the making of an appropriate recommendation by the Assembly (3). The outstanding example of this procedure was the Assembly resolution of 1 February 1951, finding that Communist China had engaged in aggression in Korea.

When East and West are not hopelessly at odds, the veto is not likely to prevent the Security Council from dealing effectively with threats to the peace. The action of this body in dealing with the problem of Palestine (Israel) was successful. Furthermore, the Council acted with dispatch and efficiency during the long-drawn out troubles in Indonesia. There, skillfully bypassing the Dutch claim that, the matter being within the domestic jurisdiction of Holland, the Council had no jurisdiction, the latter was instrumental in ending armed conflict and thus laid the basis for the grant of independence to the former Dutch colony. Intervening in the Kashmir trouble, a dangerous dispute between India and Pakistan, the Council ended hostilities and imposed a truce that, although

unsatisfactory and precarious, has lasted to this day. The basic reason why in these particular cases (and others) the Council could act successfully is extremely significant. This was the fortuitous circumstance that no clash of vital national interest separated the United States and the U.S.S.R. Thus, they could agree on a settlement. But when the Council was seized of the question of intervention in Greece, and also of the Corfu Channel case involving the United Kingdom and Albania, the Soviet Government came to the defense of a satellite. In such circumstances, its veto is a foregone conclusion, whatever the merits of the controversy. But in neither of these cases did the U.S.S.R. veto prevent resolute United Nations action: A commission was sent by the Assembly to supervise events on the Greek border; the Corfu Channel case was decided by the International Court of Justice.

Similarly, with respect to the admission of new members, no candidate for membership has much of a chance to be approved unless both the U.S.S.R. and the United States are on the same side of the fence. Thus it was easy to admit Israel, since the two superpowers were actually competing for the favor of the new state. But the United States will not vote for Mongolia or Albania, nor the U.S.S.R. for Italy or South Korea. It is precisely here that the veto has been used the largest number of times.

Looking back over the use of the veto in the Security Council from the beginning to the present, certain observations can be offered, which, in the nature of the case, can be only tentative. Many observers have come to the conclusion that the veto is a major cause of the present weakness of the United Nations. No doubt an impressive case can be made for this thesis, especially by the citation of statistics. Certainly there has been a marked reduction during the years in the activity of the Security Council. From July 1947 to July 1948, for example, the Council met 180 times; and for the same period in 1952-53, it met only 25 times. A qualitative approach to the same subject is even more impressive. It is highly significant that the five historic diplomatic settlements of the past year were accomplished outside the United Nations with little or no assistance from that organization: Iran, Suez, Indo-China, Trieste, and the recent London Agreements on the rearmament of Germany.

To ascribe the tendency to bypass the United Nations to present defects in the method of voting alone is a striking example of *post hoc, ergo hoc* reasoning. For one thing, an increased use of the Assembly was a natural development. The larger body is often preferred to the smaller, if only be-

cause it offers a bigger forum for purposes of propaganda. In fact the U.S.S.R. herself has aided and abetted the movement to use the Assembly rather than the Council.

The veto remains a very real problem, since it has contributed much to the undermining of popular support of the United Nations. The main cause for the weakening of the United Nations, however, must be sought elsewhere. And the answer is to be found in the present state of world politics. An examination of the world situation leads one to believe that the veto problem is not a cause, but an effect. It is a consequence of the cold war, the division between the East and West, and the disruption of the balance of power. Any lawyer knows that family quarrels can be settled peaceably, by negotiation within the family circle, only up to a certain point. After that the disputing members will resort to the law. This is especially true when one member is aggressive, working to destroy all the others. In the international field, however, there is no court with obligatory jurisdiction to which the disputing members can resort; hence they fall back on the precarious methods of diplomacy. At best, they negotiate outside the world organization; at worst, they form rival pacts, they rearm. It is thus apparent that the basic assumption accepted by the negotiators at San Francisco is sound; the new system can work only if the powers retain a reasonable degree of unity. Failing this, problems that, for lack of a spirit of mutual cooperation, cannot be solved within the new world organization (but could even be exacerbated by the kind of public debate prevalent today in that body) appear to have a better chance for solution outside, through independent agreement among the powers concerned. If these powers are friendly, as in the case of the members of the Atlantic Pact, they can negotiate much more successfully without the constant harrowing opposition of the Eastern bloc. If the parties are enemies, as France and China were in their dispute over Indochina, here again private negotiation outside the United Nations apparently offers the best chance for successful negotiation.

In present circumstances there should be no illusion; no revision of the Charter, however necessary and desirable, can make much of a contribution to a better world. The solution must be sought along other lines.

Collective Security in the Atomic Age

Perhaps it will help to appraise the meaning of the magic phrase "collective security" if the pages of recent history are turned back as much as 40

years to recall that on the eve of World War I the conception of collective security simply did not exist. The most that the jurists who met at The Hague in 1907, at the second of the two great Peace Conferences, could foresee as a basis of permanent world peace was a system of balance of power in which, by a delicately balanced equilibrium, the great powers would be so matched against one another that the risk of war would be too great to take. Arbitration was then the great hope for world peace. Let the great powers agree to arbitrate their disputes and then they would not be tempted to use their rival armaments. The idea of limiting those armaments appeared altogether too impracticable for serious discussion.

But World War I taught the terrible lesson that the balance of power was an unstable basis of peace, that one side or the other, fearing the growing strength of its opponent, would take the fatal step of anticipating defeat. In the calmer light of today it can be seen that Germany was not wholly responsible for the war, as the Treaty of Versailles declared, but that the true cause lay in the lack of an international organization that could have brought the rival powers together and made it possible to find a solution for the critical situation presented by the demands of Austria-Hungary upon Serbia.

It was President Wilson who took the lead in insisting that the principle of collective security must be substituted for the unstable balance of alliances and counteralliances. He was not the first to conceive the idea, but being a man of vigorous intellect and high ideals he fixed upon it as the basis for future peace and as the condition upon which the United States would agree to cooperate in an organization to maintain peace. Senator Henry Cabot Lodge had accepted the idea of a league to enforce peace but, unfortunately, when he was not invited to take part in the formulation of the Covenant of the League of Nations, he turned against the conception and was one of those chiefly responsible for the defeat of the plan.

There was nothing new about the principle of collective security which President Wilson and others sought to incorporate in the Covenant of the League. Collective security is the fundamental principle of law in every nation-state; that is, all the citizens are collectively responsible for the maintenance of peace and through their agents undertake to protect the individual citizen against attack. No citizen can stand aside and say that an act of violence committed against one of the citizen-body is of no concern to him. Violations of the law are the concern of every citizen, and unless he takes his part in suppressing them law

and order within the state would soon disappear.

It is easy to remember how stress was placed in those days on the fact that the United States herself was an example of the application of the principle of collective security to states. The Constitution of the United States guarantees to each state "a republican form of government . . . and shall protect each of them against invasion." This last clause is our national collective-security system. An attack upon any one of our states, North, East, South, or West, would bring the entire United States to the rescue; and it is on this principle, this rule of law, that individual states are called upon to discard local armaments and to trust to the armaments of the United States as a national union.

Collective security was possible in 1920. A combination of the great powers of that day would have been strong enough to make it altogether too dangerous for any one of their number to have committed an act of aggression.

Article 10 of the Covenant of the League of Nations contained a pledge on the part of the members to respect and preserve against external aggression the territorial integrity and existing political independence of all members of the League. Perhaps, the pledge was not phrased as well as it might have been, for it seemed to many persons that Article 10 guaranteed the maintenance of the *status quo*, and this gave the impression to many persons in the United States that there was no hope of remedying existing conditions of political dependence except by the only form of resistance possible, namely, armed insurrection. It seemed to many that governments were now entrenched in their colonial possessions, and that the League was pledged to defend them in their control. This was, indeed, not the fact, but it did serve as an argument for those who opposed the assumption by the United States of a responsibility for the maintenance of peace by cooperation with other nations in suppressing acts of aggression.

The years went by and the system of collective security instituted by the League of Nations was unequal to the high task undertaken. The causes for the failure of the League need not be of concern here. The question now is whether the system of collective security established by the Charter of the United Nations is a feasible one; and the test of its feasibility is, of course, the question whether there can be sufficient unity among the leading powers to assure that their combined forces will always be so much greater than the forces of any state that might attempt to commit an act of aggression that the act of aggression itself simply will not be committed.

This seemed to be the case in 1945, and when

the Charter was finally signed at San Francisco many were hopeful that a new era had at last begun, and that the cooperation of the United States in the maintenance of the principle of collective security would assure that there would be preponderant strength on the part of the members of the United Nations to defeat any act of aggression by a state in violation of the principles of the Charter. Unhappily the Charter had scarcely been signed when an atomic bomb was dropped on Hiroshima and a new phase of warfare had begun. Perhaps, the atomic bomb would not have been sufficient in itself to mark the new phase. But combined with the atomic bomb was the invention of larger and faster airplanes which could reach distant countries and, with the use of the bomb, devastate great cities in spite of whatever defenses might be built up to protect them.

A surprise attack now became something that neither the Covenant of the League nor the Charter of the United Nations had anticipated. One of the oldest rules of international law was the obligation to declare war, to give the opponent state time to consider its position and to yield before being attacked. Perhaps, the principle of a declaration of war was of relatively little value to a state when confronted with an ultimatum by a larger state; but it did at least mean that there could be no sudden and unexpected attack that might catch it unawares and make useless whatever defenses it had planned.

What is left today of the rule of a declaration of war? Is it to be believed that a state that has at its disposal vast and powerful airplanes and atomic bombs to drop upon great cities will, if it is determined to commit an act of aggression, declare beforehand what its demands are and announce that unless they are met within a certain time the attack will begin? No sensible man will expect any such thing. What can be expected, and what, of course, is feared, is that a surprise attack will take place simply because the advantages of it will be so overwhelming that they make resistance almost impossible. In the presence of this hard fact, this cruel fact, those who fear such an attack are themselves engaged in developing their military power so as to be able to return blow for blow and wreak the same destruction upon the enemy that the enemy is seeking to wreak upon it.

Under such conditions, there is not much left of "collective security" in the sense in which it was visualized in 1920 and on 26 June 1945, when the Charter of the United Nations was signed. Secretary Dulles announced several months ago the policy of depending primarily "upon a great capacity to retaliate instantly, by means and at places

of our choosing." What does this mean in terms of the constitutional power of the Congress to declare war, which has always been regarded in our country as one of the basic controls on irresponsible action by the Executive? It is not meant, of course, to criticize the statement of the Secretary of State, because it is perfectly clear that if the nation had to wait until the Congress, the members of which might, perhaps, at that time be scattered throughout the country, could assemble and take action before hitting back, there might be little left of our ability to hit back. Clearly, instant retaliation must mean that, to the Executive, must be left the decision to defend the country by every means available against such a surprise attack as is here contemplated. Under the condition of such a surprise attack, could even the decision of the Security Council of the United Nations be awaited, with the veto power in the hands of the permanent members? Clearly this procedure would take enough time to make recourse to it impossible.

What then remains? There has been created a defense group in Western Europe, expanding the Brussels Treaty and the North Atlantic Treaty and bringing together a body of states, which it is believed can be relied upon not to commit an act of aggression themselves and to use their forces only for such effective defense as Secretary Dulles had in mind when he spoke of massive retaliatory power. Perhaps, it must be left to this group and to the United States to make a decision in the name of the United Nations and to use the forces at their disposal to resist an act of aggression in the name of the United Nations as a whole.

This is, of course, not "collective security" as understood in 1920 or as understood before the invention of the atomic bomb and the fast-flying airplane. But for the time being it would seem the best that can be had. It would amount to a sort of delegation of power to the small number of states that are able to defend the community as a whole, trusting to their honor and integrity that their forces would not be used contrary to the principles of the United Nations, but only as forces of defense in the truest sense of the word.

Perhaps, this picture is somewhat pessimistic. But it is not intended to be so; it is intended only to illustrate the hard facts of the situation so that when the term *collective security* is used, it will be stated with the full realization of the situation, which, perhaps, is transient but is actually now extant.

While passing through this critical stage of what might be called dislocated collective security, it is necessary to strive to strengthen the United Nations along other lines and to seek to build up

within the international community such a great body of common interests, political, economic, social, cultural, and in a broad sense, human, that no one state will be tempted to defy the international community and resort to force to obtain its objective. This, perhaps, is the greatest task before the United Nations as an organization, namely, to build up reserves of moral and material unity that will command the respect of the world and deter any nation that might be tempted to violate the system of collective security from making such a fatal decision.

Economic and Social Objectives of International Trusteeship

The United Nations Trusteeship System has been aptly defined as a system of national administration under international supervision. In examining the economic and social objectives of the Trusteeship System, it is necessary to have in mind the basic elements that comprise it and to see how the respective functions of administration and supervision operate and interact upon one another.

The basic elements in the system are the 11 trust territories themselves—seven in Middle Africa and four in the Pacific—which include a population of about 20 million people. These 11 territories have all been taken from enemy states at the ends of the two world wars and, with the exception of Italian Somaliland which is the only new trust territory established after World War II, are administered by the United Kingdom, France, Belgium, the United States, Australia, and New Zealand.

There are some 58 other nonself-governing territories, commonly called colonies or protectorates, which are governed by the same administering powers but which fall outside United Nations supervision under trusteeship. The Charter provides that such territories can be voluntarily placed under trusteeship, and it is, perhaps, a weakness of the Trusteeship System that none of these 58 colonies has so far been placed under it. Although it is not relevant here to seek the reason for this apparent anomaly, one reason undoubtedly is that the administering authorities do not really feel that there would be any advantage to the colonies or to themselves by placing them under trusteeship.

The fundamental objective of international trusteeship is, of course, a political objective, namely, the progressive development of these territories toward self-government or independence. There arise, however, two rather fundamental differences between the administering authorities and the other members of the United Nations. The first difference relates to the time factor in which

self-government or independence of these territories may be attained; the second difference turns on the question of whether the economic and social development of these territories should reach a certain stage of advancement before self-government or independence is to be recognized.

United Nations supervision consists basically of two elements. The first may best be described as a kind of police function—supervision directed to seeing that the administering authority is not engaging in exploitation or abuses, such as have sometimes been practiced in past colonial history. The second is supervision of a more positive kind expressed in terms of cooperative efforts on the part of all members of the United Nations toward the solution of various economic, social, and political problems that inevitably arise. The administering authorities can hardly be expected to be very appreciative of the police function, since all of them belong to the democratic peoples of the Free World who do not honestly believe that they require policing. As for securing cooperative help in the solution of economic and social problems, an examination of United Nations debates shows that the critical, rather than the constructive, note tends to dominate the discussion. And this may be a further reason why the International Trusteeship System has not, in practice, been geographically extended to include other nonself-governing territories.

Even if they do not feel the need or the utility of international trusteeship, and even if they are disinclined to place any of their colonies under the system, the administering authorities, nevertheless, are aware that public opinion in recent times has become increasingly critical of the colonial relationship, and they regard international supervision, therefore, as a necessary feature of international relations. Indeed, it was the administering members themselves at San Francisco who took the initiative in devising the system, and it was they who voluntarily drafted the terms of the Trusteeship Agreements, which were later accepted, with very little change by the United Nations. It cannot be said, therefore, that the system was imposed upon them, except, perhaps, by the force of public opinion, or that the objectives of the system were established against their will.

The basic concept that underlies international trusteeship is that there are peoples who are not yet able to stand by themselves under the strenuous conditions of the modern world and that "the well-being and development of such peoples should form a sacred trust of civilization." This idea of the "sacred trust" was first mentioned in connection with the League Mandates System and the term

sacred trust itself was carried over into the Charter of the United Nations. Indeed, the Trusteeship System is, in essence, a projection of the Mandates System.

Positive objectives expressed in the League Covenant and in the United Nations Charter usually begin with the words *to promote*. For example, in these instruments the administering authorities are to undertake to promote the economic and social well-being of the inhabitants, to promote measures of development, to encourage research, to insure equal treatment in social, economic, and commercial matters for other United Nations members, and to promote freedom of conscience and religion and to encourage respect for human rights and fundamental freedoms.

Negative objectives are usually expressed by clauses beginning with the words *to prohibit* or *to prevent* and include such objectives as the prohibition of the arms traffic and the liquor traffic, the suppression of the slave trade and the prohibition of forced or compulsory labor except for essential public works or services, the prohibition of abuses, particularly abuses pertaining to alienation of land or other forms of exploitation, and the prevention of concessions having the character of a general monopoly.

The Trusteeship System has been shown by experience to be better adapted for dealing with the so-called "negative objectives," such as the prevention of abuses, than with the positive objectives, such as the building of roads, railroads, harbors, schools, and hospitals. The reason, of course, is not difficult to understand. The Trusteeship Council is not in a position to raise funds for economic development. The Council can and does give advice—for example, on the best methods of overcoming illiteracy—but it is not in a position to raise funds or to guarantee funds for a long-term economic development. It gives advice without corresponding responsibility. On the whole, therefore, the supervisory instrument is not well-adapted to positive reconstruction and development. The Trusteeship Council Visiting Missions, which, unlike the Mandates System, are enabled to go into the territories and examine local conditions, undoubtedly have exerted influence in preventing possible abuses, injustices, or inequities.

The Trusteeship Council, as a kind of watchdog for civilization, has, perhaps, less work to do today than would have been the case in an earlier period when colonial practices were less enlightened and less concerned with native welfare. It may be regarded in some respects as a belated reflection of a previously bad conscience that had resulted from earlier forms of exploitation, now hap-

pily largely nonexistent. Today the main emphasis of the nonadministering members is rather on making more rapid strides toward self-government or independence, whereas the emphasis of the trust powers is more likely to be on the means and methods of economic and social development which they regard as the necessary foundation for a stable and durable self-governing existence. Since some of the nonadministering members of the Council consider that self-government is a condition that should be granted at an early date, if not immediately, even if economic and social conditions are not yet what they should be, it will be seen easily that in the pursuit of the various objectives of the Trusteeship System, the members of the United Nations do not always agree on the basic premises by which the objectives may best be realized. On the question of the time factor, Secretary of State Dulles has clearly expressed the United States attitude when he said:

The United States has not wavered in its conviction that the orderly transition of self-government should be carried resolutely to a completion.

Thus, United States delegations are constantly pressing for the earliest practicable attainment of self-government, and where suitable for independence, but they recognize that orderly transition means that there should be adequate foundations of a political, economic, and social character that will enable such peoples to assume effectively the responsibilities for self-government.

The question arises why the desired economic and social objectives in the trust territories are realized relatively slowly, especially since the trust territories may be regarded as wards of the United Nations and have the particular attention of some of the wealthiest countries as administering authorities.

A number of factors enter into the answer to this question. First of all, in the primitive conditions prevailing in most of these territories, there is almost no basis for the formation of capital from internal sources. The people follow primitive methods, they live on a subsistence economy, almost entirely agricultural, and in some cases pastoral and even nomadic. Their soils are often depleted and eroded, and, with few exceptions, mineral resources appear to be largely lacking. Although European administrators have been in some of these territories for more than half a century, this is still too short a period in which to transform a primitive agricultural community into a diversified economy based upon exchange and trade relations.

The assertion has also been made that the trust

territories are at a disadvantage with ordinary colonies in the matter of their economic and social development. Risk capital, it is said, is somewhat less attracted to the trust territories for at least two reasons. The status of the territory is more uncertain, since the administering authority does not claim sovereignty over it, and there are constant pressures from the United Nations for the early attainment and, perhaps, even premature attainment, of self-government or independence. Hence, investors are more hesitant to take the additional risk necessary in long-term economic development. It may also be true that the investor does not entirely enjoy operating under the inquisitive and suspicious eyes of 60 nations when he finds it difficult enough to operate when the supervision is less pervading and exacting. In any event, for whatever the reason, funds for economic development in the trust territories have been provided almost entirely by the administering authority and its nationals. In recent years some comparatively small contributions have also come from the United Nations Technical Assistance and United States Mutual Security sources.

There are several major social objectives of the Trusteeship System that are identical with those sought by the administering authorities in territories outside the Trusteeship System. It is a well-recognized principle that economic development and social development are interdependent and that it is unrealistic to attempt to build a sound economy while neglecting the social aspects of the problem. Indeed, it can be said that economic development cannot proceed very far unless progress is also made in the social field.

The first efforts of administering authorities and supervisory bodies have been directed toward providing health conditions. To this end it is generally agreed that attainment of better health standards depends on such factors as better nutrition, more adequate housing and sanitation, and better health education. These objectives, however, can be attained only as poverty is overcome by higher wage standards, which in turn depend on higher levels of production, the opening of new channels of communication, the development of trade, and a change from a subsistence to a cash economy.

It is fairly easy to agree in theory on objectives and on the means of their attainment. But to execute them presents tremendous difficulties, since it often involves the disruption and even the breakdown of tribal customs and of primitive forms of social organization. In many instances it involves the problem of migratory labor where the head of the family will be absent from his tribe for many months in order to work in a mine or factory, with

all the resultant social problems that this brings about.

A question frequently discussed in United Nations bodies has been whether economic and social development of nonself-governing territories should depend solely on the territory's own resources and on such private investments as might come from abroad, or whether the administering members should provide funds necessary to speed up the pace of this development, and whether such assistance should be in the form of grants-in-aid or in repayable loans. Most administering authorities have now accepted the responsibility for providing funds that go beyond the immediate capacity of the territory itself to supply. It is not considered desirable, however, that such funds should tend to mortgage the long-run future of the territory. In this connection the United States has received some criticism in the United Nations for the amount of funds it is expending in the Trust Territory of the Pacific Islands on the grounds that such expenditures are creating standards that the territory can never maintain by itself.

Social objectives also concern problems of education and labor conditions. With respect to education, the problem of overcoming illiteracy, the problem of whether to teach in the vernacular or in a modern language or both, the problem of providing mass adult education, and the problem of retaining literacy, once acquired, have been given close attention, especially in the last 10 years. The objectives have generally been agreed upon, but the methods of attaining them are still subject to differing opinions. Some authorities have undertaken to provide widespread primary and elementary education rather than to select a few individuals for university training. The question of the relative merits of technical, as compared with liberal, education and the question of, for example, too many lawyers as compared with doctors and engineers, are still vexing problems in most territories. In addition, there is the problem of the native who studies in a Western university returning to his territory and not fitting into the life of his tribe and, thus, becoming discontented and, perhaps, an agitator.

The administering authorities have maintained an adequate record in the field of labor standards. Wage rates in comparison with Western standards are shockingly low, but in relation to the productivity of the worker they are, perhaps, more reasonable. In most cases, trade-union organizations, usually affiliated with those in the metropole, are overcoming unreasonable disparities. The Trusteeship Council examines closely any tendencies that might show wage differences in employments or

professions based on race. Recommendations concerned with this problem frequently emanate from the Council.

As a whole, the idea of the sacred trust is still the underlying motive of trusteeship. The last areas in the world to be touched by modern civilization are rapidly moving toward the capacity to become self-governing and, where suitable, independent. The advances made in these respects by the Gold Coast and Nigeria, for example, which carry with them similar advances in the neighboring Trust Territories of Togoland and the Cameroons, are only forerunners of what is forming in the remaining nonself-governing areas of the world.

The colonial relationship has come into disrepute and is the subject of much criticism. No people really enjoys being ruled by an alien authority, even when that rule is benevolent. This relationship, however, is rapidly changing, and in this evolution the role played by the United Nations as representing the world community is significant, even when it is not always measurable. The period immediately ahead can anticipate a rapid increase in the number of countries that, through federation or in an independent status, will become new members of the family of free and independent nations. It is only to be regretted that while this remarkable development is taking place in the Free World, the trend is so largely in the opposite direction in other areas of the world.

Economic Cooperation and the Underdeveloped Areas

Countries in which levels of national and family income are low and the dominant industry is agriculture are commonly referred to as "underdeveloped" by the representatives of such countries at the United Nations. *Underdeveloped* should always be understood to be a relative term, since development is a matter of degree. No country is completely undeveloped and none is ever fully developed. Moreover, in many highly developed countries there are areas that are relatively underdeveloped.

Economic cooperation is used here to mean the help in accelerating the rate of economic development that comes from outside the particular country. Such help may be in many forms and may be carried on in many different ways. Among those in use—some of them very old, others of very recent origin—are (i) individual payments to relatives overseas and contributions to the relief of sufferers of earthquakes, floods, and famines; (ii) the provision by private nonprofit foundations of technical aid and research in the problems of health, agriculture, and technology of underdevel-

oped countries; (iii) the private investment of capital abroad; (iv) technical and economic aid by governments; (v) loans by governmental bodies; (vi) the United Nations Technical Assistance Program; and (vii) loans by the International Bank for Reconstruction and Development.

The interest of industrially advanced countries in underdeveloped areas has grown strikingly during the period since the close of World War II. The debates and proposals in the United Nations have undoubtedly been influential in stimulating such interest, but it has grown elsewhere. The manifestations of interest include such operations as the United Nations Technical Assistance Program, the Colombo Plan, and numerous programs of bilateral cooperation, notably those of the United States under which about 6.5 billion dollars have gone in aid to underdeveloped countries, mostly economic aid.

The reasons why industrially advanced countries are prepared to make some financial or other sacrifices in order to help promote economic development, as these reasons appear in the debates of the United Nations General Assembly and the Economic and Social Council, are of three kinds. First is the moral imperative, the weight on the conscience of the more developed countries that results from an awareness of the growing divergence in levels of living between the wealthier and the poorer countries, and of the poverty, hunger, and disease so often present in the latter. Second are the gains in political stability that it is hoped will accompany economic development. In many countries with low levels of production and consumption, the growing awareness on the part of the masses of people that a better life is possible makes them unwilling to accept without political protest a continuation of conditions of hunger and misery. The raising of per-capita production, which is a major aspect of economic development, thus, may be a vital factor in achieving or maintaining political stability in such countries. Third are the economic benefits that the industrially advanced countries themselves may expect to derive from promoting the economic development of the less-developed countries. Certain specific industries, of course, may be adversely affected by the growth of the same industries in other countries. That the economy of the United States as a whole, however, will be benefited by accelerated economic development of the underdeveloped countries, even where this takes the form of rapid industrialization, is a proposition that would, perhaps, command the support of a great majority of economists. An expanding world economy is the best assurance that economies of particular countries will continue to

expand. Among the many factors that determine the volume of international trade carried on by a country is the level of its income. Generally speaking, the larger the per-capita income of a country the more, per capita, it buys and sells in international markets.

Thus, moral, political, and economic reasons combine to make the speeding up of the rate of economic development in underdeveloped countries an objective that industrially advanced countries seek to promote. Effectively administered aid by industrially advanced countries is, accordingly, not necessarily a "give-away" but rather an investment. The returns on this investment, although not always definitely traceable, may nevertheless be high.

Implicit in the willingness of people to cooperate in the economic development of other countries is the belief that the rate of economic development in a country or region depends on the extent to which certain factors are present or absent, and that enough of these factors can be consciously and deliberately affected, within a framework of democratic institutions, to accelerate substantially the rate of development. The factors that in combination determine the rate of economic development of a country are very numerous. Many of them are intangibles, such as the procedures and practices of public administration, the moral tone of the public officials and of the population generally, the social prestige of work and of entrepreneurial activity, and the available supply of trained labor and managerial talent. The rate of domestic savings and the extent to which they are devoted to productive investment often are vital.

Representatives of underdeveloped countries and of industrially advanced countries alike recognize that some internal factors may, in the given situation, constitute obstacles to rapid economic development. In the industrially advanced countries this recognition has seemed in some cases to be coupled in the public mind with an attitude of insistence that the underdeveloped countries "put their houses in order" by removing internal obstacles before external cooperation is granted. It would, indeed, seem not too much to ask that the will to eliminate internal obstacles to development should be present in the underdeveloped country and that positive steps be taken to do so, but it is a counsel of perfection to insist that all major internal obstacles actually be removed before external cooperation is offered, especially in view of the positive interest of the industrially advanced countries in promoting economic development in underdeveloped countries. The situation may involve deeply ingrained practices and attitudes and wide-

spread lack of understanding of their relationship to economic development. No simple act of the government is likely to correct such a situation, at least in the short run, and, indeed, it may for the time being be impossible to secure public assent to the needed changes. External cooperation may be necessary to the creation of conditions of growth and development under which the needed internal changes may gradually be made.

The representatives of the underdeveloped countries emphasize three general kinds of factors on which they believe international cooperative action is required to accelerate the rate of economic development. One such factor is assistance in bringing to their people the knowledge and benefits of modern techniques—commonly referred to as technical assistance or technical aid. Technical aid in many cases can achieve tremendous results at relatively small cost. In most circumstances, however, acquiring technical knowledge is not sufficient to give a satisfactory rate of economic development, since the use of the more modern technical methods is likely to require much new and additional capital. The difficulties of creating large amounts of capital from the low levels of income and in the conditions prevailing in many underdeveloped countries make necessary substantial international movements of capital to achieve a rapid rate of development. While different persons place different emphases on the relative importance of public capital and private capital, they are in the main complementary rather than competitive. Before private capital will be invested in some countries, it is necessary to provide an adequate foundation of roads, public health, education, and the like. These can scarcely be made to yield a commercial return and, therefore, require the investment of public capital. For these reasons the international flow of public capital is, in general, a factor encouraging the investment of private capital. Of course, it is not impossible that some specific investments of public capital may be competitive with some possible specific investments of private capital.

Although underdeveloped countries with few, if any, exceptions are eager to receive private capital, they have found it desirable to insist that the investment be in harmony with the needs of the country and that the political and economic "sovereignty" of the country be protected. These viewpoints have led to some misunderstandings, but to an increasing degree they appear to be recognized by investors in industrially advanced countries as both inevitable and reasonable.

In the United Nations in recent years the General Assembly and the Economic and Social Coun-

cil have been pressing for the establishment of two new international instruments, one to enlarge the flow of private capital and the other to enlarge the flow of public capital. The proposed International Finance Corporation would have an underlying capital fund of internationally contributed public capital, but would seek to function by investing in private industries of underdeveloped countries and passing the securities on to private investors in the industrially advanced countries. There seems to be some prospect that this corporation may be realized in the relatively near future. More resistance has continued to be shown by the representatives of industrially advanced countries to a proposal for establishing a special United Nations fund for economic development. This fund would be financed by contributions from industrially advanced countries and would be distributed to the underdeveloped countries in the form of grants-in-aid or long-term, low-interest loans.

A third type of international action for which the representatives of underdeveloped countries have been pressing in the United Nations and other international organizations involves the designing and application of measures to prevent in the future wide fluctuations in the prices of primary products relative to the prices of manufactured products. Such fluctuations have been chronic in the past and have proved to be extremely damaging to the economies of the countries that rely heavily on the export of agricultural products and other basic raw materials. Underdeveloped countries must usually rely on their foreign exchange earnings from the sale of primary products, not only to purchase many necessary consumer goods, but also to finance the purchase of capital goods required for development. The more favorable the terms of trade, the larger the volume of foreign exchange available for foreign purchases. Fluctuations in terms of trade lead to an alternating feast and famine situation that demoralizes business and workers and can seriously interfere with economic development plans. As a step toward meeting this problem, the Economic and Social Council has established an 18-member Commission on International Commodity Trade, which is expected to hold its first meeting in January 1955.

Programs of technical aid, loans, and grants carried on unilaterally by countries on the basis of agreements with recipient countries are substantially coordinated with programs of international organizations, and the two types complement each other. In a sense, however, they are competing methods, each having its advantages and disadvantages. The attractiveness to the contributing

country of giving its aid unilaterally and, thus, maintaining full control is so evident that many persons may not be aware that the use of an international organization also presents advantages. For one thing, the international body has many members and can draw on a much larger source of funds than can even the wealthiest country. Moreover, the contributions of one country may be, in effect, the instrument for securing contributions from other countries, thus, enlarging the total volume of funds available. Another advantage of using an international organization in the giving of technical assistance is that it permits the recruiting of technical assistance experts from many different countries. This is often of great advantage to the countries that receive technical assistance, since it makes possible the use of technical knowledge best adapted to their needs.

Another advantage of international programs is their cooperative character. The United Nations Expanded Technical Assistance Program supplies a good illustration. One form of cooperation in this program is that which exists among a number of agencies within the United Nations family, each specializing in the kinds of technical assistance for which it is particularly qualified, but all coordinated into the program as a whole. In the second place, and more important, the program is cooperative in that it is not simply a matter of two rigid categories of countries, some giving aid and others receiving aid. Almost every country has special technical knowledge that may be usefully applied in other countries so that a country is very

often both the recipient and the supplier of technical aid at the same time. During the first 4 years of the existence of the program, the experts were of some 68 nationalities.

Finally, the attitude of the underdeveloped countries that receive aid deserves consideration, since it is in the impact of the aid on these countries that the purpose of assistance programs is to be found. Countries receiving aid appear generally to prefer to have it come through international bodies. Using an international organization as the instrument for granting aid greatly reduces the embarrassment of being an aid recipient and avoids both the danger of political domination by the contributing country and possible charges that such domination exists. These are factors that are significant also to the contributing country. The advantages are such that, as they become more widely understood, the use of international organizations for the provision of technical and economic aid would seem likely to increase. The future of these functions of the United Nations, therefore, presumes a considerable potential for international economic development.

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A Physician's Avocation

This month's cover is a photograph of a sculptured bust of Abraham Lincoln that was done by Emil Seletz. Seletz is assistant professor of neurosurgery at the University of Southern California School of Medicine, chief of neurologic surgery at Cedars of Lebanon Hospital, and senior attending neurologic surgeon to the Los Angeles County General Hospital. He is author of *Surgery of Peripheral Nerves*. In portrait sculpture his subjects have been principally famous physicians. However, his favorite subject has long been the face of Lincoln. The original life mask at the Smithsonian Institution was intimately studied for this piece of sculpture. At present Seletz is working on a bust of Albert Einstein for the new medical school in New York bearing his name, and also on another head of Lincoln some 3 ft in size.

Sperm Maturescence

DAVID W. BISHOP

Dr. Bishop is a staff member of the department of embryology, Carnegie Institution of Washington. He received his training at Swarthmore College and the University of Pennsylvania, supplemented by fieldwork in the Panamanian Cordilleras, in the Colorado Rockies, and at the Woods Hole Marine Biological Laboratory. During World War II he served as an aviation physiologist in the Army Air Force. He has taught at the universities of Pennsylvania, Colorado, and Illinois and at Swarthmore College and has been a visiting professor at the California Institute of Technology. His research in reproductive physiology includes investigations of the cytology, metabolism, and motility of mammalian germ cells.

CONTRARY to long-standing assumption, mammalian spermatozoa are not ready for their role in fertilization when they are released from the testis. At this early stage, these sexual cells are far from complete in their physiological development. Since the first realization of the significance of spermatozoa in reproduction, these minute bearers of hereditary transmission have been generally regarded, on cytological grounds, as fully functional, capable of fertilization, and otherwise mature, regardless of where they might be found in the male or female genital tracts. However, it is now apparent that the male germ cells demand a period of time and modification before their full measure of fertilizing capacity can be realized. This potentiality is progressively approached while the spermatozoa are undergoing profound physiological transformations during their passage from testicle to fertilization site.

The recent recognition of this "ripening" process in spermatozoa—maturescence—has stimulated both research into the nature of prefertilization gametic changes and exploration of new areas of attack on the ever-present problems of fertility and sterility. It is unlikely that much improvement can be achieved toward devising intelligent and infallible procedures of contraception or for the regulation of conception without first having a more complete understanding of the physical and biochemical conditions in the sexual cells and in the fertilized egg. This is true, whether the objective be that of the clinician who is called upon to recommend such procedures, or that of the sociologist who stands appalled at the awesome increase in birth rate in those many quarters of the world where a limitation of population might ameliorate the enigmatic social and economic imbalances.

When puberty is reached in the male, spermatozoa are produced in prodigious numbers in the

testes, and this cell formation persists, though gradually waning, throughout the course of sexual maturity. Possibly a trillion (10^{12}) such sexual cells are generated during a human life-span. A continuous procession of spermatozoa passes from the testis into the fine tubules of the epididymis and thence into the larger collecting duct, the vas deferens, of the genital tract. At intervals the paired vasa are forcibly emptied of the spermatozoa stored within, and at each ejaculation the cells are intermixed with seminal plasma from the accessory glands—the seminal vesicles, the prostate, and the bulbo-urethral, or Cowper's, glands (Fig. 1).

At insemination, the male gametes are introduced into the vagina and the cervix uteri. As a result both of autonomous sperm movement and the muscular activity of the tract, the cells migrate through the uterus and finally into the oviduct. Fertilization generally occurs in the upper segment of this duct when a single spermatozoon from the many millions introduced fertilizes the ovum shortly after its rupture from the ovary.

This thumbnail sketch is made despite the extreme variation that exists in the details of the reproductive processes from species to species: rate of sperm transport, periodicity of ovulation, number of ova shed, survival times of the gametes, time of fertilization, structure of the genital tracts, and the like.

The point of view that spermatozoa undergo fundamental alterations after their escape from the testes was inferred two decades ago from a series of enlightening breeding experiments in which spermatozoa from different levels of the male genital tract were used for artificial insemination. Ordinarily, of course, only the older, mature spermatozoa from the distal portions of the ducts are introduced at insemination—a high fertilization rate thus being assured. However, both

in fish and in mammals, the fertilizing capacity was found to increase markedly when these cells were siphoned off from points farther and farther down the tract (1, 2). Since no obvious structural modifications occur in the slow passage through the tubes, the changes in the cells must be of a functional nature—alterations that improve their chances for successful reproduction. Although these early controlled excursions into animal breeding were accomplished with chickens, rats, rabbits, and guinea pigs, a comparable situation is now believed to exist in human spermatozoa.

Furthermore, it has recently been established that spermatozoa must also spend a minimum of several hours in the female genital tract, where further changes are undergone. For example, if ejaculated rabbit spermatozoa are immediately introduced directly into the open fimbriated end of the oviduct of an ovulating female under anesthesia, these cells are incapable of completing fertilization until 4 to 6 hr have elapsed (3). However, if the spermatozoa are first "aged" for approximately 5 hr in the uterus of a foster doe before they are similarly introduced into the oviduct, they are then immediately capable of fertilizing ova. Likewise, when rat spermatozoa are collected from epididymides and injected into the membranous sac surrounding the female gonad, they cannot engage in fertilization until after they have remained there approximately 4 hr (4). Here too, then, a delay involving some kind of sperm metamorphosis is required.

What nature of change—gain, loss, or transformation of substance—is responsible for these indications of sperm maturation? The final answer, not immediately forthcoming, would be a detailed account of the process of physiological differentiation in one of the most highly metabolic and complex types of cell in the animal organism.

The number of spermatozoa that are present at the time and site of fertilization bears witness to the fact that the going is not easy. For only a few out of the many millions of cells introduced complete the journey. No "sperm swarm," but only a fraction of the total number of cells normally deposited can be counted in the vicinity of the eggs at the time of fertilization—about ten in the rat, and several hundred in the rabbit (5). On scant evidence the same seems to be true in monkeys and in women. Presumably the failure of many spermatozoa to reach their predestined goal lies in their lack of vigor and in their sensitivity to inimical conditions along the route. Effective obstacles to migration do exist, particularly at the uterotubal junction and at the cervix, where many cells are screened out and abandoned. In fact, many of

the spermatozoa of rodents and monkeys are trapped irretrievably in the *bouchon vaginal* produced at copulation by the enzymatic gelation of seminal constituents.

With some modifications in detail, the structural pattern of spermatozoa has been reasonably well standardized by nature. Generally, a spermatozoon consists of a head with its nuclear and genetic elements, a mid-piece which is vitally concerned with energetics, and a long, tapering flagellum that is responsible for motility (Fig. 2). The application of electron microscopy to the study of male germ cells has resulted in the demonstration of a maze of intricacies that indicates more basic conformity than variation in intracellular plan (Fig. 3). The electron microscope has just begun to be applied to the problems of microscopic changes in prefertilization development of spermatozoa. But some significant alterations have already been shown in the gametes of certain marine invertebrate animals such as the sea urchin. When these spermatozoa, for instance, are exposed to exudates from the ova, abrupt morphologic changes occur in the head (6). These alterations in the presence of egg-substance, "fertilizin," are believed to represent an important event in gamete interreaction in the early stages of fertilization (Fig. 4).

Certain gross morphologic irregularities in mammalian spermatozoa have been pointed out as being indicative of anomalous cell formation or

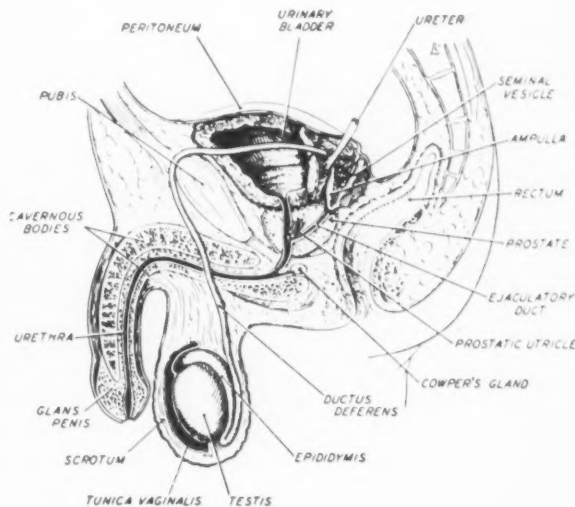


Fig. 1. The human male genital system, schematically represented. Immotile spermatozoa, produced in the testis, are stored within the vas deferens and the distal lobe of the epididymis. Upon ejaculation the sperm are mixed with secretions from the seminal vesicles, the prostate, and Cowper's gland, and only then do they normally become motile. [From C. D. Turner, *General Endocrinology* (Saunders, Philadelphia)]

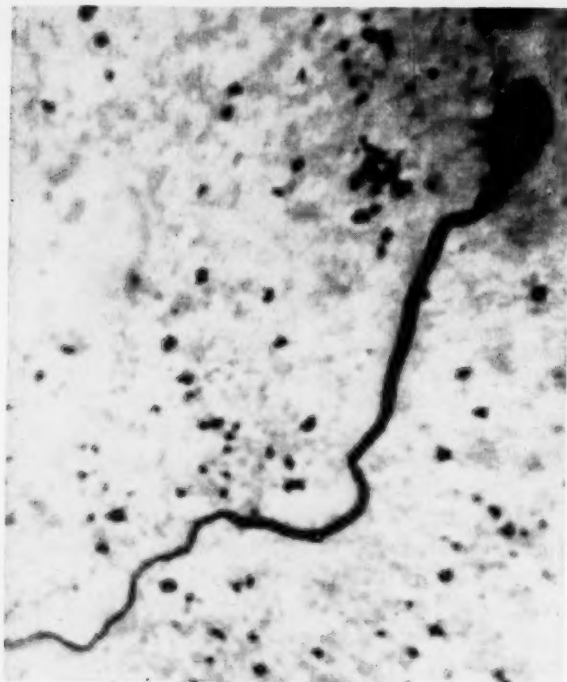


Fig. 2. Human spermatozoon stained by the silver (AgNO_3) impregnation method and magnified 1700 times. The head, containing the nucleus with its chromosomal-genetic hereditary complement, appears banded owing to the anterior cap, the galea capitis. The "neck" is small and beaded, whereas the mid-piece blends almost imperceptibly into the extremely long tail. A million of these cells could rest side by side on the head of a pin. [From D. W. Bishop, "The Metabolic Machinery of Sperm Activity, in E. T. Engle, Ed., *Studies on Testis and Ovary, Eggs and Sperm* (Thomas, Springfield, Ill.)]

degeneration. Thus occasional superfluous protoplasmic remnants attached to the sperm tail indicate inadequate streamlining and precocious shedding of cells from the testis. Such gametes are presumed to be nonfunctional, as are old cells, which through excessively long storage may lose their morphologic integrity by the fracture or loss of tail tip or head-piece, the *galea capitis*.

The physical changes that accompany normal maturation in spermatozoa are less obvious than the afore-mentioned aberrant structural changes and are essentially on a molecular order of magnitude. During the passage of the sexual cells through the male genital tract, for example, the individual cells become denser and show an increase in specific gravity; water seems to be literally squeezed out as it becomes less tightly bound by the surface lipids, which themselves undergo molecular reorganization (7). Reabsorption of water is known to be one of the functions of the mucosal lining of the epididymis (8).

These changes in surface lipids and in the water content of the maturing gametes parallel and may

account for the gradually decreasing resistance to sudden thermal reduction—"cold shock"—in older spermatozoa. This phenomenon was discovered by, and continues to be of importance to, the animal husbandry industry. Refrigeration of spermatozoa, suspended in nutritive diluent, at a few degrees above freezing is still the preferred technique for storage, despite the growing success in vitrifying spermatozoa for long periods at the exceptionally low temperature of Dry Ice or liquid nitrogen.

The idea of sperm storage, however, is not something confined to an icebox—merely the preservation of chilled semen for practical purposes, valuable as this may be. Actually, storage of spermatozoa begins in the male genital tract, the difference being that here, *in situ*, slow physiological changes occur, and at the temperatures of the animal body and of the scrotum. Indeed, the cells may spend many days in transit through the male ducts. Carefully controlled experiments have clearly shown, for example, that the trip through the narrow but extensive tubules of the epididymis requires about 2 days in the fowl, close to a week in the rabbit and ram, and at least a fortnight in the guinea pig and in man (1, 9).

Experiments to determine how long stored spermatozoa can remain capable of motility or of fertilization within the male genital ducts have clearly shown that fertilization capacity is invariably lost first, and that sperm motility therefore is not a valid criterion for fertilization capacity. Thus, although rat and guinea pig spermatozoa trapped in the epididymis retain fertilization capacity 3 to 4 weeks, they may remain potentially motile for a period twice that long (10).

The survival times of mammalian spermatozoa in the male genital tract generally far exceed those in the female tract. Here, the duration of survival is usually 1 or 2 days, depending on the particular species and the phase of reproductive cycle. Exceptions are known, to be sure, in the functional persistence of male sexual cells, but these are limited to birds and to such quasi-hibernating mammals as bats.

The survival of spermatozoa—the duration of motility and of fertilizing capacity—reflects the environmental conditions of the various sections of the male and female genital tracts—the sources of energy substrates (sugar, phospholipid, and so forth), hydrogen ion concentration, oxygen tension, and the like. The greatest single factor that characterizes the responses of spermatozoa, however, lies within the cells themselves, namely, their ability to adapt rapidly to changes in their biochemical environs, to vary their metabolic pattern, and to utilize different available substrates. The

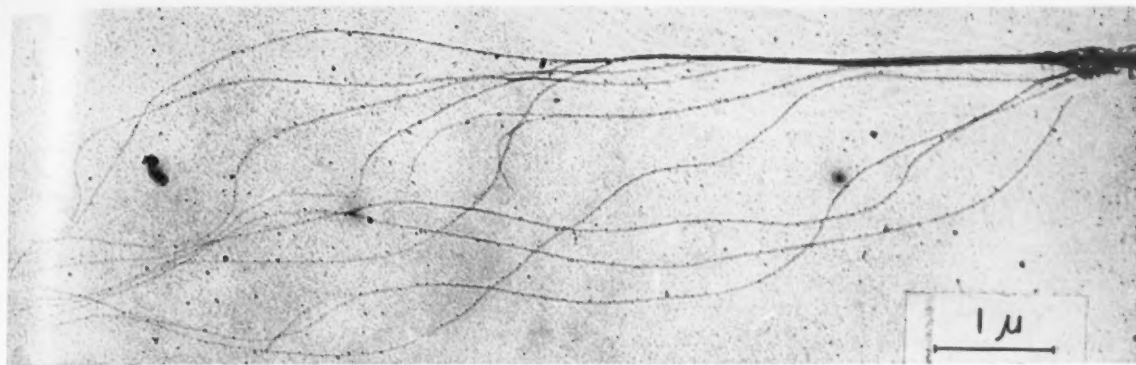


Fig. 3. The end of the human sperm tail fixed in 1 percent osmic acid vapor and demonstrated in an electron micrograph prepared by Richardt Hammen of Copenhagen (16,000 \times). Ten prominent but slender axial filaments, which may themselves split, project from the spiral sheath. These filaments extend the length of the flagellum. The axial filaments in all vertebrate and invertebrate sperm range from nine to eleven in number.

explosive increase in metabolic activity at ejaculation represents a shift from a pattern characteristic of immotile, long-lived cells to that characteristic of extremely active, short-lived, energy-devouring gametes. With a veritable unleashing of metabolic controls and the temporary acquisition of exogenous energy substrates, the spermatozoa are irrevocably directed into a path of self-imposed exhaustion.

The immotility of spermatozoa within the male genital tract is an energy-saving device *secundum naturam* and results from a combination of three, or perhaps more, factors: the reduced oxygen tension, the absence of any appreciable quantity of sugar, and the restraints upon the adenosine triphosphate energy-transfer system. This series of checks and balances discourages both aerobic oxidation and anaerobic glycolysis and prevents motility. The frequently suggested role either of carbon dioxide accumulation or of high acidity as an inhibitor of sperm motility in the tract is, at best, of minor importance.

When freed from the limiting confines of the vas deferens, as at the moment of ejaculation (or removed from the epididymis and suspended in saline solution), the gametes rapidly become vigorously motile. With metabolic curbs removed and an adequate supply of hexose available from the accessory gland secretions, a high rate of sugar utilization—that is, glycolysis—is instituted (Fig. 5). By artificially excluding sugar and by substituting for it—with phospholipid, for example—in *vitro*, glycolytic activity may be replaced by oxidative respiration. In either instance, sperm motility is assured by the energy made immediately available by the enzymatic breakdown of adenosine triphosphate, the labile compound that also plays a pivotal role in muscular contraction (11). Since fructose is the primary energy substrate in the

semen of most mammals, the supposition is that after insemination this sugar constitutes the cornerstone for a glycolytic type of metabolism. However, since only moderate, if any, quantities of sugar can be demonstrated in the female tubal fluids and an indeterminate but limited amount of seminal fructose is carried into the female genital tract, it is sheer conjecture to assign, as yet, a metabolic pattern to inseminated spermatozoa. An oxidative metabolic process, even one that ultimately utilizes the stored phospholipid reserves of the spermatozoa themselves, may provide the necessary energy for the 1- to 2-day survival within the female tract.

Various agents have been utilized *in vitro* to retard the runaway metabolism of seminal spermatozoa and thereby to extend their length of survival. This is of importance, of course, when semen is required for artificial insemination. Certain

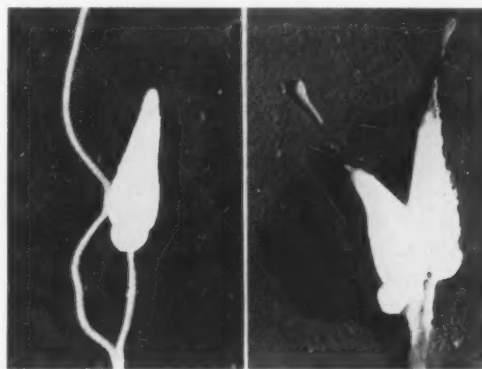


Fig. 4. Extrusion of anterior tip of head of the sea urchin sperm (right) treated with eggwater containing fertilizin derived from the eggs of the same species. After such change the sperm is no longer capable of fertilization. Untreated sperm shown on the left. Photograph supplied through the courtesy of J. C. Dan, of the Misaki Marine Biological Station, Japan.

chemical reagents, such as triphenyltetrazolium which temporarily interrupts vital enzyme reactions, are moderately effective (12). But the best tool in this connection is certainly the lowering of temperature. The fertilizing capacity of bull spermatozoa, for example, is extended from a day to a week by refrigeration in nutrient mediums. The modern technique of vitrification, moreover, by which spermatozoa are reduced to the extreme subzero temperature of Dry Ice, liquid nitrogen, or liquid helium, appears to be a way of storing sperm perhaps indefinitely (13). When thawed after such treatment, the sexual cells of man and those of many other mammals have proved capable of fertilization. As a physical inhibitor of meta-

bolism, this method is without parallel. Just what happens to cells maintained indefinitely at, say, -269°C , the temperature of liquid helium, is open to speculation; so treated, "the immortality of the germ plasm" literally becomes a physical reality!

Both significant and useful are these procedures for controlling *in vitro* the activity of sexual cells. More engaging at the moment are the metabolic controls imposed on the spermatozoa resident within the genital tract. Recent work at the University of Wisconsin has given us a better understanding of what may transpire within the cells and what changes activate them at the time of ejaculation. The seminal spermatozoa, both of the

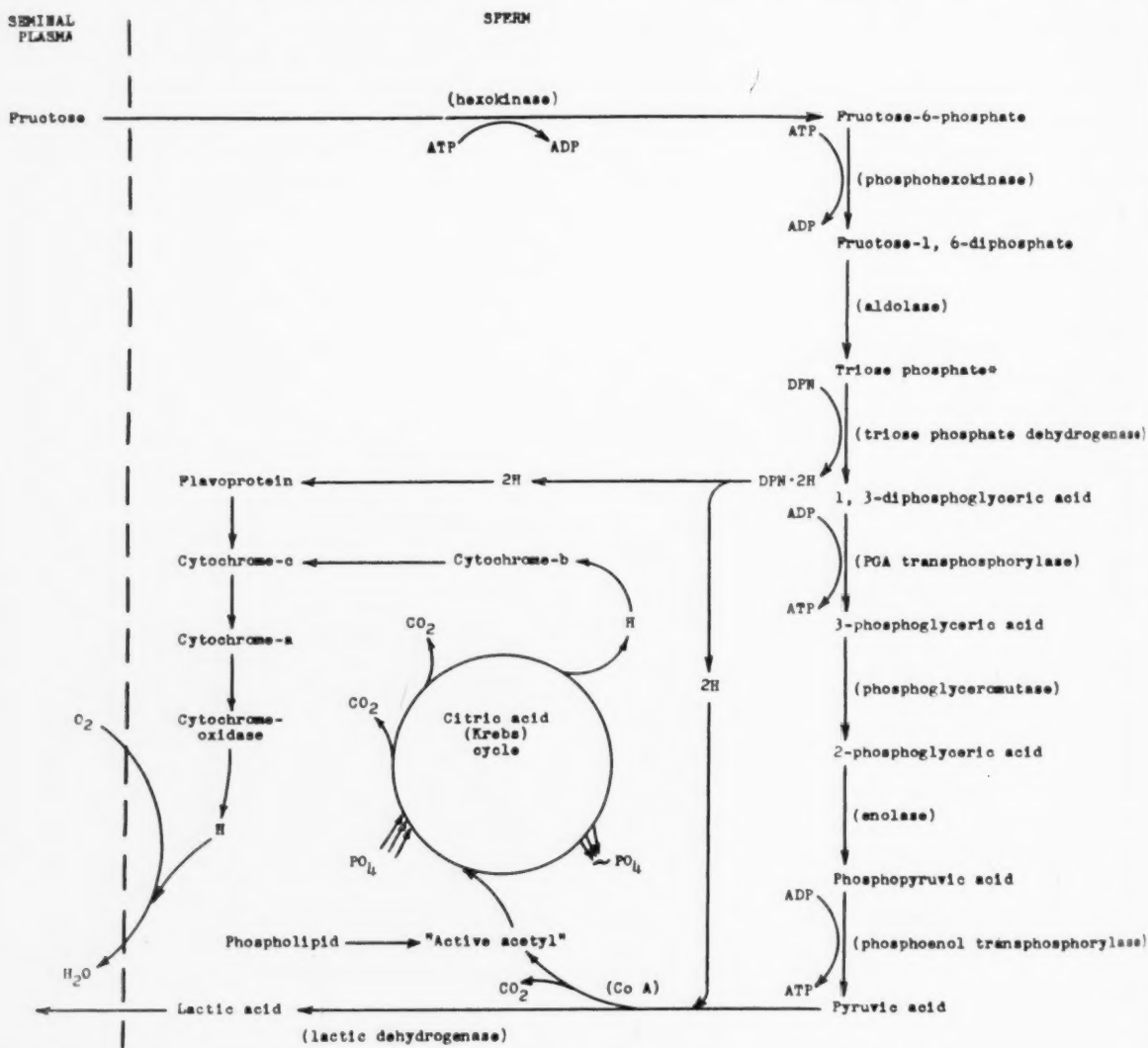


Fig. 5. The probable glycolytic and oxidative pathways occurring in mammalian spermatozoa. The critical enzyme systems and substrates have been demonstrated *in vitro* in suspensions of bull and sheep sperm. All reactions are, theoretically at least, reversible.

* Triose phosphate is a mixture of 3-phosphoglyceraldehyde and dihydroxyacetone phosphate.

rabbit and the bull, were shown to have a higher endogenous oxygen uptake and utilization of exogenous sugar as compared with spermatozoa removed from the epididymis. This increased activity results from the stimulation by a compound, apparently a polypeptide with active sulfhydryl groups, that is bound in an inactive form in epididymal spermatozoa and is gradually liberated enzymatically after ejaculation (14). The addition of this "metabolic regulator" to suspensions of epididymal spermatozoa renders them more active and therefore more like seminal spermatozoa. Thus the gradual release of substance from the cells themselves increases their rates of metabolism and motility and, incidentally, sets in motion the final catabolic spurt which must end either in destiny or demise.

The loss of "metabolic regulator" is not the only material decrement from spermatozoa as they approach fertilization. Specific enzymes also appear to be cast off from the cells. The enzyme hyaluronidase, for instance, acquired while the spermatozoa are in the testes, is gradually lost from the surface as the spermatozoa migrate through the genital tracts. This does not contradict the fact that some of this much-discussed enzyme is still present at the time of fertilization, thus enabling the gametes to penetrate the cumulus oöphorus, the residual layers of follicular cells surrounding the ova. But much of the enzyme has been lost in transit. Cytochrome *c* is also leached from the spermatozoa as they age, but sufficient quantities remain to enter into the oxidative reactions of the cells.

Just as some complex surface substances are removed from the spermatozoa during maturation, and others are modified, certain reactive groups seem at that time to make their first appearance. Such an aggregate is that of the agglutinating substances of rabbit spermatozoa which are here at the time of fertilization. The sperm agglutinins can interact with water-soluble complementary substances derived from rabbit ova, with the result that the motile spermatozoa are agglutinated, in a manner similar to the response of invertebrate spermatozoa to "fertilizin" (Fig. 6) (15). These demonstrations of sperm agglutination in the presence of egg substances, *in vitro*, suggest a serologic factor in the initial stages of mammalian fertilization. Although this specific reaction is obvious in suspensions of seminal spermatozoa, it is less intense in preparations of germ cells removed from the epididymis. The agglutinating complexes appear to be either exposed or activated with age and transport through the genital tract.

Sparse as the evidence is and incomplete as the

tentative explanation may be, the fact of sperm maturation is clear and its meaning significant. Important is the understanding that many modifications—physiological and invisible—can and must take place in the structurally differentiated, so-called "mature," male germ cells. Estimations of functional quality, based on morphologic criteria, are at best only crude approximations.

Measures taken to improve semen quality—that is, to favor sperm transport and conception—must reckon with the protean nature of spermatozoa and of their interactions with their environment. By the same token, further steps adopted to limit conception by interfering with the normal activities of the male gametes are more likely to become strides if the diversity of sperm activity is appreciated. As with these *relevant*, if not in all ways *practical*, considerations, a comprehension of the gradual attainment of fertilizing capacity by spermatozoa is paramount in approaching such technical problems as attempts at fertilization *in vitro*.

With regard to an explanation of the possible underlying causes and mechanics of the various aspects of maturation, we can as yet only hazard calculated guesses. The predominant changes occur only *in situ*; at least our methods of sperm culture are not yet satisfactory to provide, *in vitro*,

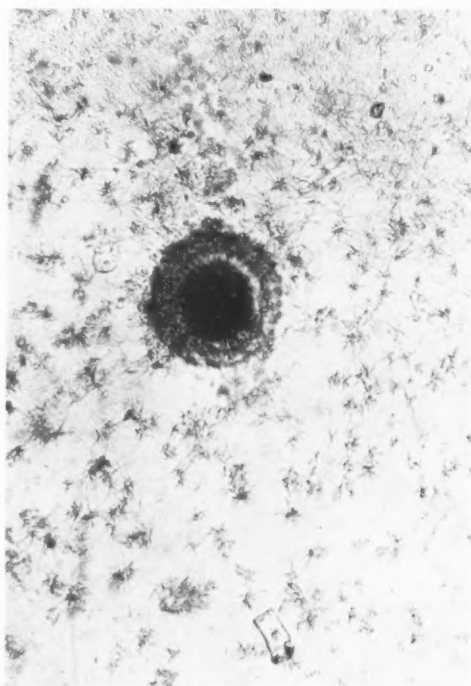


Fig. 6. Agglutination of rabbit spermatozoa by exudates from the rabbit egg, shown in center. This reaction is irreversible but is otherwise similar to the usual reaction of invertebrate sperm to fertilizin.

for the modifications that occur within the organism. That some of the changes are intrinsic characteristics of the gametes is apparent; others reflect the interactions of the spermatozoa and their immediate surroundings. Until these mutual relationships are more clearly understood the problems will be before us.

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Enrico Fermi: 1901-1954

The flame that lit great Galileo's eye
and flashed his mind to Jupiter's pale moons
burned bright in Fermi, searching another sky,
reading the script of subatomic runes,
piercing the atom's whirling outer shell,
making the meson's nuclear secret clear,
kindling plutonium fire, flinging a hell
of fissioned flame beyond the stratosphere. . . .
He never faltered, never failed the faith
that knowledge gained will serve the human quest
to nurture life and slay the ancient wraith
of fear, that man will meet his final test.
Knowledge and love: man's best—and only—hope:
Science reveals the cryptic horoscope.

—READ BAIN

Science as a Social and Historical Phenomenon

These four articles—by H. Guerlac, E. C. Boring, A. Koyré, and R. S. Cohen—are based by the authors on papers read by them as part of the symposium on "Science as a social and historical phenomenon" that comprised the final part of a five-part conference on the general subject Validation of Scientific Theories held in Boston, Massachusetts, in December 1953. Gerald Holton of Harvard University was chairman for the symposium.

The papers for the preceding four parts, on "Reasons for the acceptance of scientific theories," "The present state of operationalism," "Psychoanalysis and scientific method," and "Organism and machine," appeared in the September, October, November, and January issues.

Some Aspects of Science during the French Revolution

HENRY GUERLAC

Dr. Guerlac, who is professor of the history of science at Cornell University is presently on leave at the Institute for Advanced Study, Princeton, New Jersey. He was trained in biochemistry at Cornell and took his doctorate in European history at Harvard University. In 1941 he founded the department of history of science at the University of Wisconsin and from 1943-46 served as historian of the OSRD radar research.

THE late 18th century, including the period of the French Revolution, is a richly rewarding field of study for anyone concerned with the influence of science upon society, or with the impact of social change upon the work and thought of scientists. Never before, and rarely since, has science enjoyed such unalloyed esteem as it did in the Europe, especially the France, of the Age of Enlightenment, when it had for its advocates and propagandists the outstanding men of letters and social theorists from Montesquieu and Voltaire to Condorcet and Volney. Inspired by the writings of Descartes and Newton, these men drew confident arguments from the realm of physical law in their campaign to bring a similar rule of reason, law, and harmony into the inherited social institutions of their day. From science and its recent history, moreover, they took their most compelling examples of intellectual progress, finding support therein for their gilded vision of indefinite human perfectibility.

This favorable climate of opinion helps explain the mysterious concatenation of events discussed in this paper: the fact that the greatest period of

French scientific leadership coincided almost precisely with the Age of Revolution; and that the time of Mirabeau, Danton, Robespierre, and Bonaparte was also, I need hardly point out, that of Lagrange, Laplace, Monge, Condorcet, and many other illustrious names in mathematics, physics, and astronomy; of A. L. de Jussieu, Lamarck, Cuvier, and Geoffroy Saint-Hilaire in botany, zoology, and paleontology; of Bichat in physiology and anatomy; and of Lavoisier, Berthollet, and the other French founders of modern chemistry.

What is difficult to comprehend—especially in view of the persistent tradition that the spirit of the Revolution was detrimental, if not actually antagonistic, to science—is that this scientific flowering was not fatally arrested or totally destroyed by the distractions of the Revolution, by the bloodbath of the Terror, by the mounting wave of emigration, or by the endless wars of the Republic, the Consulate, and the Empire. Yet this was clearly not the case. The scientific generation of the Napoleonic period and the Restoration—that of Arago, Poisson, Magendie, Gay-Lussac, Sadi Carnot, Cauchy, Fresnel, and the rest—is as rich if

not richer in talent than the generations that came before. Yet we look in vain for truly comparable achievements in the art, the music, or the *belles lettres* of this revolutionary period; and we are forced to the conclusion that the national energy and the great social ferment that overthrew the Old Regime, spreading a new democratic gospel across Europe by flaming word and glinting bayonet, found its greatest cultural expression in scientific accomplishment.

Science and its practitioners played a notable role in the intellectual preparation for the Revolution as well as in the seething events of the Revolution itself. What this may have amounted to I can only summarize, in full realization of the complexity of the problem and the monographic work that remains to be done. My main purpose in this essay is to examine in preliminary fashion what happened to scientific progress, to scientific institutions, and to scientists themselves during the great Revolution, and to offer a general picture, tentative at best, that may help awaken the interest of other scholars in these problems and reveal, perhaps through my own errors, the gaps in our knowledge.

I

In the decade before the Revolution, European science felt the loss of such outstanding figures as Euler, Linnaeus, Daniel Bernoulli, and the great northern chemists, Scheele and Bergmann. In France, d'Alembert died in 1783 and Buffon in 1788. Despite these losses, this was throughout Europe a time of extraordinary productivity and promise in the world of science, with France and England unquestionably in the lead, but with Switzerland, Italy, the German states, and Scandinavia boasting many proud names. As if to put the seal on France's acknowledged leadership, the eminent mathematician Lagrange, a native of Turin who for 20 years had been the beacon light of the Prussian Academy, left Berlin in 1787 after the death of Frederick the Great and took up his residence in Paris. It was here that he published in the following year his great *Mécanique analytique* under the auspices of the Royal Academy of Sciences. In 1789, the year of Revolution, there appeared three of the acknowledged classics of French science: A. L. de Jussieu's *Genera plantarum*; Philippe Pinel's *Nosographie philosophique*; and Lavoisier's epoch-making *Traité élémentaire de chimie*. Each work in its own domain—botany, medicine, and chemistry—was both a fulfillment and a new departure.

The swiftly moving events of the Revolution's first phase—the convening of the Estates General, the disorders in Paris and the provinces, the aboli-

tion of inherited privilege, the creation of a constitutional monarchy—found the scientists neither aloof nor unprepared. Having taken an active part in the liberal movement of the previous decades, they welcomed the first phase of the Revolution with enthusiasm; indeed men like Bailly, Condorcet, and Lavoisier had played their modest part in bringing it about. Politics already infringed upon science, disturbing the tranquility of the laboratory and penetrating the fastnesses of the Academy of Sciences. On 4 July 1789 the Academicians took the unprecedented step of expressing to their fellow-member, the astronomer Bailly, their satisfaction at the manner in which he had performed his duties as president of the National Assembly; and later in the month the members of the Academy went in a body to Bailly's residence in Chaillot to pay their respects to him. Yet on the day following the storming of the Bastille the Academy held its regular meeting with 23 members present; technical papers were presented, and there is no echo in the *procès-verbaux* of the storm raging without (1). Throughout the remainder of 1789 and well into 1790, fundamental scientific questions continued to dominate the meetings of the Academy (1). Laplace read a series of important papers on celestial mechanics; Coulomb presented his sixth memoir on electrostatic experiments with the torsion balance; Lavoisier and Seguin reported on their classic experiments on respiration and heat regulation in man and other animals, the last work on pure science carried out by the senior partner (2). There was great interest in current English work in observational astronomy; money was even set aside to build a great reflecting telescope, modeled upon the great instrument with which the great English astronomer, William Herschel, was busy charting the nebulae and observing the rings and satellites of Saturn (3).

But concern about the effect of the revolutionary tensions and of the obsession with political events is reflected in the correspondence of the scientists. In August 1789 the chemist Berthollet wrote to James Watt (4).

While you are occupied tranquilly with science and the useful arts which owe you such great obligations, we have been obliged to lose sight of them. The ferocity of the great nobles, the insurrection of the citizens, the fury of the people, the scourge of famine have absorbed all our attention; yet one must return to peaceful occupations, and one can begin to enjoy the pleasures of study. I am taking up my experiments once more.

And the mathematician Gaspard Monge, toward the end of the same year, commented to the same correspondent (5):

Revolution occupies every mind, each in his own fashion; and science is the loser. May God bring it to a swift conclusion, for we shall lose the habit of work and the love of science.

Lavoisier wrote in like vein to the Scottish chemist, Joseph Black, lamenting the interruption of scientific activity and expressing the hope that calm and prosperity would soon allow the scientists to return to their laboratories (6). While from Chaptal, the founder of French industrial chemistry, we have the following cautious appraisal of the opportunities and dangers that lay ahead (7): . . .

The revolution which is taking place is a beautiful thing, but I wish it had arrived twenty years ago. It is annoying to find oneself under a house that is being torn down, but that is precisely our position. . . . In this general confusion, in this torrent of passions, the intelligent man studies the role he should play: but it seems just as dangerous to remain outside of the excitement as to participate.

As these letters indicate, the scientists were being inexorably drawn into the revolutionary turmoil. In 1788 Lavoisier had prepared a long memorandum on the proper constitution of the Estates General; early in 1789 he took a major part in drawing up the *cahier* of grievances to instruct the representatives of the nobility of Blois; and in May he was chosen alternate deputy to the Estates General. The astronomer Bailly was one of the political leaders of the first Revolutionary assembly; and shortly afterward was chosen the first mayor of Paris. The mathematician Condorcet, Perpetual Secretary of the Academy of Science, plunged at once into the journalistic and political activity that led to his election—along with such other scientists as Tenon, Lacépède, Fourcroy, and Guyton de Morveau—to one or another of the succeeding assemblies. Other scientists were active in the Paris sections, or Revolutionary districts and served in battalions of the National Guard.

By the time of the acceptance of the constitution in September 1791, the moderates—and they included all but a handful of the younger scientists—hoped, as we have just seen, that the violent and disruptive phase of the Revolution was at an end and that the time had come to plan constructively for the future. To this end there was founded in April of 1790 a short-lived but influential association called the Society of 1789, which aspired to be the intellectual guide and official planning agency of the new society and its elected assembly. Besides well-known liberals and reformers of the pre-Revolutionary period—Brissot, Dupont de Nemours, Mirabeau, Talleyrand—it included influential members of the Academy of Sciences: Con-

dorcet and Lavoisier; the mathematician Gaspard Monge; the biologist Lacépède and Lamarck, and others (8). Briefly, from June through September 1790, this society published a journal, edited by Condorcet, which is our only direct evidence of what transpired in their meetings and to the philosophy that pervaded them. The society's avowed purpose (9) was to aid and promote

. . . all discoveries useful to the progress of *l'art social*, to encourage those being made in these sciences themselves and to gather together suggestions relative to public institutions that may be formed for public welfare and for education.

A perusal of this short-lived journal shows that, besides treating in rather high-flown and abstract language such basic social problems as the rights of women and the proper foreign policy for a free nation, it devoted space to the discussion of the national economy and the importance of a scientific technology (10). Lavoisier read a famous paper on the *assignats* and the inflationary dangers of a paper currency. The chemist Hassenfratz wrote on the importance of promoting the useful arts; contributed a long article on the mineral resources of France and the possibilities of developing them; and described in another number recent advances in chemical industry: LeBlanc's famous soda process and Berthollet's use of hypochlorites for bleaching.

In the discussions of this society are to be found, I believe, the germs of many of the constructive revolutionary accomplishments: the various efforts undertaken to stimulate productive industry and invention; possibly also the great reform in weights and measures and the creation of the metric system although this had earlier antecedents; but above all, although direct evidence is lacking in the *Journal*, the plans for new educational and scientific institutions, such as those later elaborated by Talleyrand and Condorcet.

The philosophy of science, or rather of social science, that guided these men is worthy of a moment's attention. It centers upon Condorcet's conception of a unified social science, an *art social*, based upon a collaboration and unification of the sciences according to a common spirit and, where possible, a common methodology. The vision of what Comte was to call *sociology* is clearly discernible in the manifesto of the society drawn up by Condorcet (11):

There should exist for all societies a science of maintaining and extending their happiness: this is what has been called *l'art social*. This science, to which all others are contributors, has not been treated as a whole. The science of agriculture, the science of economics, the science of government

. . . are only portions of this greater science. These separate sciences will not reach their complete development until they have been made into a well-organized whole. . . . And this result will be obtained sooner if all the workers are led to follow a constant and uniform method of work.

If one asked of Condorcet how such a unification of the sciences could be brought about, how the experimental and mathematical spirit of the natural sciences could be transferred to the sciences of man and society, he would not have agreed with the early system-builders of the 18th century, or with John Stuart Mill, that it is enough to build aprioristic deductive systems in imitation of the great scheme of classical mechanics. Condorcet placed his faith in what he called social mathematics, embodying the twin disciplines of social statistics and mathematical probability, subjects to which he had contributed, together with Laplace and Lavoisier, in the years before the Revolution. Just at this time he was preparing a popular exposition of social mathematics in his *Elémens du calcul des probabilités*, for he saw in it a useful instrument of social improvement and reform. In his preface he explains why he feels that social mathematics was at this moment indispensable; and he continues (12):

When a Revolution has ended, this method of treating the social sciences takes a new direction and acquires a greater degree of utility. In fact, to repair promptly the dislocations inseparable from every great movement, to restore general prosperity, one needs stronger methods [than mere argument], means calculated with greater precision, supported by unattackable proofs in order to ensure the adoption of needed reforms in the face of selfish interests and base faith.

We hardly need to remind ourselves that when Condorcet wrote the Revolution had not ended but was moving with torrential rapidity toward greater confusions and dangers, in which science and scientists alike suffered. Later men like Quetelet, Cournot, and Auguste Comte in the 19th century were to pick up, each in his own fashion, the prophetic program that Condorcet was obliged to abandon.

II

Even before the outbreak of war in 1792, the demands of a succession of revolutionary governments upon the Academy of Sciences and its members left little time for normal activities. By all odds the most time-consuming and exacting responsibility—overshadowing such requests as that the Academy examine and test silver vessels taken from *ci-devant* churches and recommend the

proper method of reconverting the secularized church bells of bronze and bell-metal—was the great project for the standardization and rationalization of the system of weights and measures. By a decree of 8 May 1790 the National Assembly charged the Academy of Sciences with determining the best scheme, based upon some universal standard found in nature, that might be adopted by all nations. Early in 1791 the Academy recommended a decimal system of units, derived from a unit of length, the meter, to be established by geodetic measurements. After a favorable report by Talleyrand to the Assembly, the Academy was assigned the task of making the basic measurements and preparing a reliable set of primary standards. This involved a long and tedious series of operations, still not completed when the Academy was abolished in August 1793.

Despite this drain on its personnel and energy, the Academy continued regular sessions until the summer of 1793. Even in the final 6 months of its existence, regular meetings were still being held, although the exigencies of national defense and the mobilization of science for war—one of the earliest such phenomena in history—sometimes reduced the participants to a mere handful. The Academy even continued its practice of announcing the subjects of annual prize contests (13). A subject proposed for the year 1793 is of special interest. The prize was to be awarded for the best theoretical analysis of the operation of steam engines, with a discussion of methods for their improvement (14), surely one of the most important technical problems of the time. No prize was actually awarded, although it was again announced in 1793 for the year 1795, no memoirs having been received, and the problem was finally attacked for the first time a generation later by Sadi Carnot, the son of the man who in these years was organizing the victory of the Republican armies.

During this crucial period the collapse and destruction of many of the venerable scientific institutions had an equally damaging effect upon the progress of science. The *Imprimerie Nationale*, now flooded with job-printing for the government, could no longer serve, as it had throughout the 18th century, for the publication of scientific books. Important serial publications, among them the *Journal des savants*, the *Mémoires* of the Academy of Sciences, and even the newly founded *Annales de chimie*, were suspended because of lack of funds or contributors. But the most serious blow was the suppression, in August 1793, of the venerable Academy of Sciences.

The detailed story of the Academy's fall has yet to be written, and I shall not attempt it here. Its

ultimately the fate, and that of the other royal academies, was heatedly debated from 1790 to 1793. While the monarchy lasted, eloquent voices were raised to preserve it virtually unchanged, but vitriolic attacks had already begun, both within and without the assemblies, demanding its immediate abolition as an aristocratic remnant of the past, and as a "school of servility and falsehood." Effective pamphleteers, chief among them J. P. Marat, attacked the Academy and its members unmercifully in the public press. As far as I have been able to judge the plan most widely favored—for example, by men like Talleyrand and Condorcet—was to effect a peaceable transformation of academies, including the Academy of Sciences, into learned bodies more acceptable to the new climate of opinion; until this could be effected it was hoped to continue the Academy virtually unchanged. These tactics were very nearly successful, as they proved to be in the case of the Jardin du Roi, which emerged enlarged and strengthened as the Muséum d'Histoire Naturelle, a research center of great importance. Disagreement within the Academy of Sciences brought delay, and delay was fatal. While defending the academies before the Legislative Assembly in 1791 the Abbé Grégoire made known that the academies were, of their own accord, reforming their statutes to put them in harmony with the new era and erasing traces of their monarchical past. There is evidence that such a draft of new statutes was actually prepared by the Academy of Sciences, but it seems to have been without effect (15).

On 8 August 1793, Grégoire read to the Convention a report on behalf of the Committee of Public Instruction in which he proposed the suppression of the academies, in order to reorganize these bodies, as he put it, in the light of human wisdom and progress. The Academy of Sciences alone, by virtue of his special utility, was to escape suppression. But the Convention was in no mood to brook exceptions; after a vituperative speech by the painter Louis David, the Academy of Sciences was extinguished with the others. The members were even denied the privilege of constituting themselves a Free Scientific Society and of using their accustomed meeting place in the Louvre. The doors were closed and sealed; soon after, these echoing chambers were invaded by an army of tailors, busy stitching uniforms for the Revolutionary armies, while workers removed the last vestiges of monarchical symbolism from the walls.

Several years were to elapse before the constructive plans of the Convention replaced or successfully remodeled the older scientific institutions. Under the Directory, harried as it was by inflation

and war, there nevertheless were miraculously established those scientific institutions which were to be the boast and pride of France during the succeeding century: the Institut de France, the Ecole Polytechnique, the Conservatoire des arts et metiers, the brilliant but ephemeral Ecole Normale, the Muséum d'Histoire Naturelle. The result of prolonged planning and debate, going back at least to the discussions in the Society of 1789 and in the early Revolutionary assemblies, the final formulation of these plans must be credited to the Convention, the most ruthless and determined of the Revolutionary governments. This fact was conveniently forgotten by the writers and propagandists of Napoleonic days, who left the impression that Bonaparte, almost singlehandedly, had saved French science, which the Jacobins had sought to destroy.

Soon after the collapse of the Academy, organized scientific work came to a virtual standstill. Under the Jacobins' iron rule, the Republican Conservatives—the Girondist opposition—were herded to public execution in the Place de la Révolution. The astronomer Bailly joined Philippe Egalité, Mme. Roland, and lesser enemies of Robespierre in the tumbrils of the guillotine. The members of the General Farm, the tax-collecting corporation of the Old Régime, were arrested in the fall of 1793, the chemist Lavoisier among them. Tried before the Revolutionary Tribunal on 8 May 1794 and convicted of a specious charge of conspiring with the enemies of the Republic, all were executed on the same day. The scientific community stood in appalled confusion. Many, like Laplace, found hiding places in the country (it was in such circumstances, for example, that he completed his popular *Système du monde*). Trapped on the outskirts of Paris, Condorcet is said to have taken his own life with poison foresightedly obtained from his friend Cabanis, the physician and philosopher (16). Yet it is astonishing to record that no scientist of note joined the flood of emigrés, which reached its peak in these years. Against none of the scientists, moreover, can a charge of counterrevolutionary activity be seriously maintained. On the other hand, few of Lavoisier's erstwhile coworkers—not Fourcroy, Guyton-Morveau, Monge, or Berthollet, all of whom were serving the Convention—raised a voice in his defense, nor had they openly protested the arrest of Bailly. Political passion, fear, and perhaps personal resentments may explain, but cannot condone, this conduct.

After 9 Thermidor (27 July 1794) when Robespierre fell, sanity returned and the scientists could survey the wreckage. The vandalism toward the

scientific institutions, the execution of Bailly and Lavoisier, were at once held up as among the most abominable of Robespierre's crimes. Condorcet was accorded his apotheosis as patron saint of a new learned publication, the *Décade philosophique* founded in 1794, organ of the so-called "Idéologues." In Millin's *Magasin encyclopédique* there appeared, the year following the execution of the Farmers General, the first biographical sketch of Lavoisier, a factual but moving account by his long-time friend, the astronomer Lalande (17). The same year a somewhat nauseating memorial service was held for Lavoisier at the Lycée des arts, the main feature of which was a ponderous eulogy by Fourcroy, the erstwhile Jacobin, who sought to defend his failure to aid Lavoisier (18).

III

It is of some interest that even during these critical years, and before the creation of the new scientific institutions, a thin but persistent thread of scientific activity is clearly evident. Private initiative took over where the public institutions gave way or were destroyed. The old Lycée de Paris, a center for public lectures founded in 1780, took on considerable importance until it was shunned for harboring men suspected of antirevolutionary proclivities. Its more scientific and utilitarian competitor, the Lycée des arts, was founded in 1792 and flourished through the darkest days of the Terror. It stressed the application of science to the useful arts, and among its outstanding lecturers were Fourcroy, Berthollet, Daubenton, and Jussieu (19). Millin's *Magasin encyclopédique*, begun in 1792 but not firmly established until 1795, gives a picture of its activities and was in some respects its organ.

Of more importance for fundamental scientific work were two new societies whose rebirth, in one case, and prosperity, in the other, were due to the conditions I have described. The first of these, the Société d'histoire naturelle, had been founded, only to disband, in 1788. It was revived after the Revolution, and an English commentator wrote of it as follows in 1793 (20).

... the disadvantages to which it was exposed, in common with the non-privileged societies, under the old government, and the jealousy of some of the protected literary bodies [i.e. the Academies] soon caused its dissolution.

After the revolution, its founders, however, reunited, extended their plan, and instituted the present Society of Natural History, which was joined by all the naturalists of the capital. . . . The object of their labours is Natural History in

general, but especially that of France, and in particular of the environs of Paris. . . . New researches are to be made by means of periodical excursions taken by the Society, either in the country, at the proper seasons of the year, or to gardens, museums, etc. . . .

This is rather too peaceful and bucolic a picture for this period of general harassment, but it is certain that the society became genuinely active after Thermidor. It was frequented by the professors of the Muséum d'Histoire Naturelle, which had early made its peace with the Revolution and where substantial scientific work was being accomplished by men like Lacépède, Lamarck, and Cuvier. Significant papers by these men and others were published in the society's *Journal*, which appeared briefly in 1792, and in its *Mémoires*, first published in 1799 (21).

A second and distinctly more important scientific society owed its inception, like the Natural History Society, to the fact that it was no longer necessary to obtain royal approval (and, in addition, at least the passive acquiescence of the Academy of Sciences) for a society holding scientific meetings and issuing a regular publication. This was the Société philomatique, which played a very useful role in the scientific life of this troubled period and has continued to this day. Beginning in 1788 as an informal discussion group of six almost unknown physicians and scientific amateurs, it was joined, in September 1789, by the young chemist Vauquelin and a few others. These men constituted themselves a regularly organized scientific society, with dues, correspondents, and the project of publishing a monthly *Bulletin* or journal. Its membership increased slowly between 1790 and 1792, but as yet it attracted no important scientists. But suddenly, after the suppression of the Academy of Sciences, distinguished names were added to the roster. In 1793 the Société philomatique was joined by Berthollet, Fourcroy, Monge, Lamarck, and Lavoisier, that is to say, by the acknowledged luminaries of the *ci-devant* Academy of Sciences. Between the time the Academy was abolished in August 1793 and the autumn of 1796, when the Institut de France was formally established, the Société philomatique was the principal haven of the dispossessed scientists. It maintained close ties with the Société d'histoire naturelle, and with the scientists at the Muséum. After Thermidor, its president referred to it as the only society officially recognized as having offered during the period of terrorism "un point fixe de réunion aux sciences et aux arts" (22).

In 1791 its *Bulletin* was launched as a monthly

summary of scientific progress that was circulated in manuscript among the members. In 1792 it was printed in a few copies but consisted only of short abstracts. Its first real issue as a learned journal is that of April 1797. The printer, of whom we must say a few words, was the economist and publicist, Dupont de Nemours, erstwhile member of the Society of 1789 and close friend of many scientists.

Private initiative once again filled the gap left by the loss to the Academy of Sciences of the facilities of the Imprimerie Nationale, which during the 18th century had printed the official publications of the Academy, and many individual works of science bearing the seal of its approval. Private printers like the Jomberts and the Didots had made something of a specialty of scientific printing during the 18th century, and they were by no means inactive during the Revolution. But the man who should be notable for coming to the aid of the scientists in this capacity during the period of crisis is Dupont de Nemours. In June 1791, as he was leaving the Constituent Assembly, Dupont issued a prospectus informing the public that he was opening a well-equipped publishing house where, he said, he proposed to do "good and inexpensive work for those who are chiefly interested in the contents of a book" (23). This venture of Dupont's is well known, but it is usually assumed that he printed only political tracts and his *Correspondence patriotique*. Nor is it widely known that the Lavoisiers, husband and wife, were among his sponsors (24). From them Dupont borrowed the sum of 710,000 francs for the purchase of his printing house. It is therefore not surprising to find that Dupont had a share in publishing works of his earlier associates in the Society of 1789. In 1791 he helped bring out Talleyrand's famous report on public education and printed Lavoisier's *Etat des finances de France*. In 1793 he published the *Réflexions sur l'instruction publique*, which Lavoisier drafted in the name of the Bureau de consultation des arts et métiers.

A number of scientific books also appeared over Dupont's imprint: the first edition of Fourcroy's *Philosophie chimique* (1792) and a second edition in 1795; a treatise by Antoine Portal on tuberculosis (1792); two editions of Daubenton's *Tableau méthodique des minéraux*; a *Flora* of the Pyrenees by Picot de la Peyrouse (1795), and other works. Dupont was the official printer for the Academy of Sciences from 1791 until its dissolution (he is so listed in the *Almanach Royal*), and in this capacity did such job-printing as the prize announcement of the Academy mentioned in the preceding section. In 1794 Dupont published the belated

volume of the *Mémoires* of the Academy for 1789 and, in 1797, the volume for 1790. He was also, as we have learned, publisher of the *Bulletin* of the Société philomatique (25).

IV

Some general remarks about the character of scientific work in this period seem appropriate here. If we cite at random, as I did at the beginning of this article, the great names that illuminated these decades, we convey the impression of distinguished and virtually uninterrupted scientific progress. A closer examination does not confirm this. The first years of the Revolution, perhaps to 1792, were still quite productive, because the momentum of the previous years was not immediately arrested. The really creative period of the men of the Revolutionary generation—men who, like Lagrange, Lavoisier, Monge, Berthollet, and Laplace, were in their forties or early fifties in 1789—falls in the years before the Revolution. Monge presented in his lectures at the Ecole Normale his great invention of descriptive geometry, but this had been worked out long before when he taught at the Ecole de Mézières. The monumental *Mécanique celeste* of Laplace did not begin to appear until 1799, but the work seems to have been well advanced by 1790 and, but for the Revolution, might have been completed much sooner. The same rule seems to hold for Legendre's *Essay on the Theory of Numbers* (1794) and his work on elliptic functions (1798), both of which grew out of earlier work.

Those scientists of the older generation who survived the turmoil of the Revolution—and they were the great majority—made their greatest contributions in this period as inspiring teachers of the Napoleonic generation. Indeed the production of brilliant pedagogic works is a marked feature of the period from 1794–1800. Monge's lectures are in this category, and so were the later books of Lagrange, based on his teaching at the Ecole Normale and the Ecole Polytechnique. Laplace's famous *Essai philosophique sur les probabilités* grew out of lectures delivered at the Ecole Normale in 1795. An outstandingly successful example is Legendre's *Eléments de géométrie*, a skillful reworking of Euclid. And on a lower plane were the immensely popular mathematical textbooks of Sylvestre Lacroix, widely used at one time in this country. Laplace's readable *Exposition du système du monde*, like his general discussion of probability just mentioned, was clearly a manifestation of a desire to present serious scientific speculation to a wide audience, yet in a spirit markedly different

from the glib popularizations of the 18th century.

A similar phenomenon is observed in chemistry, where the great textbooks of Chaptal and Fourcroy, and the latter's *Dictionary of Chemistry* in the *Encyclopedie methodique*, sought to present in intelligent order the facts of the new chemistry. The pioneer works on applied chemistry by Chaptal and Berthollet emphasize another aspect of this new orientation.

The naturalists of the Museum, chief among them Lamarck, form something of a special case. Sustained, even pampered, by the Revolutionaries of the Left, the workers at the Museum were less adversely affected by events. It was the revolutionists who called Lamarck, a man of 50 and a botanist, to the newly created chair of invertebrate zoology; and here during the subsequent years he did his best and most famous work, developing his theory of biological evolution and collecting the materials for his pioneer descriptive treatise of invertebrate zoology (1815-22).

That science lost much that the Revolutionary generation intended to accomplish is suggested by what we know of the work of leading Academicians on the eve of the Terror. Had the great reflecting telescope been built, it might have turned French astronomy into channels of observational astronomy in which, in the 19th century, other countries, including America, surpassed her. But the money set aside for this purpose was presented to the Convention as a *don patriotique* in a frantic effort of ingratiating.

From what we have recently learned of the plans and projects of Lavoisier at the time he was lost to science, we see that his creative energies had in no sense flagged. He was 46 when the Revolution broke out, and if he could have matured his scientific plans uninterrupted the results might have been incalculable. At the time of his imprisonment he was preparing a fundamental revision of his *Traité élémentaire de chimie* and an edition of his collected works; and only recently it has been pointed out that in addition he had outlined a great work of chemical theory, or, as he said, of *philosophie chimique* (26). More important still, he considered his last work on respiration and body heat control as a starting point for a research program in what we would now call medical biochemistry. A passage in Lalande's sketch of Lavoisier, which has been generally overlooked, makes this point with pardonable exaggeration (27):

By these curious and difficult experiments [on body chemistry] he had already acquired insight into the causes of different diseases and on ways of supplementing nature in their cure, and he was

preparing to attack the reversed and ancient colossus of medical prejudice and error. Nothing was more important than this work of Lavoisier; and one can say that if the sciences have experienced an irreparable loss, all humanity should join us in lamenting this privation.

Except for Bichat and some of the younger naturalists I have mentioned, the men who were between the ages of 18 and 30 in 1789 belonged to a lost generation: men old enough to have their earliest productive years blighted by the storm, too old to benefit by the great schools and the illustrious masters that prepared the Napoleonic generation. Yet the brilliance and the diversity of talent that blossomed in the first decades of the 19th century testifies to the fundamental vitality of science in Revolutionary France. Clearly there were shifts of emphasis toward a broader democratic base of scientific instruction, toward a greater preoccupation of scientists with problems of industrial application and questions of social utility. Those men of science, a small though illustrious minority, who lost their lives during the Terror, were men who had, to a large extent, given up science for politics. It was as politicians, financiers, and public officials that they were executed, not as men of science. Although the Revolution, as any such painful crisis must, produced profound dislocations, it yielded also enduring benefits in industrial progress and new scientific institutions. At no point can we simply affirm, as did the men who wished to blacken still further the men of the Revolution, that the Revolution felt it had no need of men of science.

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- society has a bound volume of the 15 numbers of the *Journal de la Société de 1789*, from 5 June to 31 Sept. 1790, together with the prospectus of the journal.
11. Gallamel, *op. cit.*, p. 393. See also M. J. Laboulle, "La mathématique sociale: Condorcet et ses prédecesseurs," *Revue d'histoire littéraire de la France* 40, 33 (1939).
 12. Condorcet, *Elémens du calcul des probabilités, et son application aux jeux de hazard, à la lotterie, et aux jugemens des hommes*, par Feu M. de Condorcet. Avec un discours sur les avantages des mathématiques sociales et une notice sur M. de Condorcet (Paris, An XIII-1805).
 13. A "prix national d'utilité" had been awarded in 1791 to the English astronomer William Herschel and in 1792 to Paul Mascagni (1752-1815) for his magnificent illustrated work on the lymphatics. See Lalande, "Histoire de l'astronomie, pour 1792," *loc. cit.*, p. 249.
 14. This prize of 1080 livres, the Prix Montyon, for the best memoir tending to simplify the processes of some mechanical art had been awarded in 1792 to a M. Girard, an engineer of Poitiers, for his study of the best method of constructing locks for canals and harbors. See *Prix Proposé par l'Académie Royale des Sciences, Pour l'année 1793*, 2 pp., de l'Imprimerie de Dupont, Imprimeur de l'Académie des Sciences, 1792 (Collection de Chazelles, Bibliothèque de Clermont-Ferrand).
 15. When this paper was delivered, the existence of these new statutes was a mere surmise. After a search in the records of the *Académie des Sciences*, in the summer of 1954, two working copies were discovered. These regulations were debated for nearly 6 months and were finally approved by the Academy on 13 Sept. 1790; they were never officially promulgated or printed.
 16. It is more likely that Condorcet died of a circulatory disorder brought about by fatigue, exposure, and hunger. See J. S. Schapiro, *Condorcet* (New York, 1934), pp. 106-107.
 17. *Magasin encyclopédique* 5, 174 (1795).
 18. *Notice sur la vie et les travaux de Lavoisier, précédée d'un discours sur les funérailles, et suivie d'une ode sur l'immortalité de l'Âme* (Paris, L'An Quatrième, 1795).
 19. Ch. Dejob, *De l'établissement connu sous le nom de Lycée et d'Athénée et de quelques établissements analogues* (Paris, 1889).
 20. "A discourse on the origin and progress of natural history in France," *Memoirs of Science & the Arts, etc.* (London, 1793), vol. I, pt. II, p. 448. On the early days of the society, see C. G. Kraft, *Notice sur Aubin-Louis Millin* (Paris, 1818), pp. 8-10.
 21. *Mémoires de la Société d'histoire naturelle de Paris* (Paris, Prairial An VII, 1799), see especially pp. iii-ix. The activities of this society can be followed in the *Magasin encyclopédique* from 1795 onward.
 22. Marcellin Berthelot, "Origines et histoire de la Société philomatique," *Mémoires publiées par la Société philomatique à l'occasion du centenaire de sa fondation, 1788-1888* (Paris, 1888), pp. i-xv. For the constituent articles and the list of early members, see the *Rapports généraux des travaux de la Société philomatique de Paris* (Paris, n.d., 1800?).
 23. See his prospectus entitled *Imprimerie de Dupont Deputé de Nemours à l'Assemblée Nationale*, dated 8 June, 1791 (*Bib. Nat.* Vp 21199). An English version of this document is published in B. E. du Pont, *Life of Eleuthère Irenée du Pont from Contemporary Correspondence* (Newark, Del., 1923), vol. I, pp. 141-145.
 24. See B. E. du Pont, *op. cit.*, vol. I, p. 185, note 1.
 25. Dupont's undated *Notice sur l'institution de la Société philomatique*, an 8-page pamphlet, probably dated from the period of the society's rapid expansion, 1792 or 1793. A copy of this was found among the Lavoisier papers (dossier 162) in the *Archives de l'Académie des sciences*.
 26. M. Daumas, "L'élaboration du Traité de chimie de Lavoisier," *Archives Internationales d'Histoire des Sciences* (1950, vol. XXIX, pp. 570-90).
 27. *Magasin encyclopédique* 5, 183, (1795).



Dual Role of the Zeitgeist in Scientific Creativity

EDWIN G. BORING

Dr. Boring, who for 25 years was director of the Harvard University Psychological Laboratory, is best known for his two books on the history of experimental psychology and for his writings on theoretical and systematic psychology. In recent years he has become interested in the psychology of the history of scientific discovery, in the way in which the changing climate of opinion facilitates and hinders scientific progress in any one period, in the role of great men in science, and in the causes of their greatness as well as the effects.

THIS "magic" term *Zeitgeist* means at any one time the climate of opinion as it affects thinking, yet it is also more than that, for the *Zeitgeist* is forever being altered, as if the thinker whom it affects were shifting latitude and longitude over sea and land so that his climate keeps changing in unpredictable ways. Goethe, who in 1827 may have been the first to use this word with explicit connotation, limited it to the

unconscious, covert, and implicit effects of the climate of opinion, at the same time ruling out thought control by such explicit processes as persuasion and education (1).

Such a concept proves useful in those cases where plagiarism is clearly unconscious, as so often it is. No man clearly understands the sources of his own creativity, and it is only since Freud that we have begun to have an inkling of how general is this

lack of understanding of one's own motives and of the sources of one's own ideas. On the other hand, this conception long antedates Freud, for it was the essence of Tolstoy's argument in 1869 that "a king is history's slave" whose conscious reasons for action are trivial and unimportant. Charles Darwin, Herbert Spencer, and Francis Galton all supported Tolstoy's view of the unconscious determination of the actions of great men, against the more voluntaristic views of Thomas Carlyle, William James, and some lesser writers.

Later the historians of science and of thought in general found themselves faced with the essential continuity of originality and discovery. Not only is a new discovery seldom made until the times are ready for it, but again and again it turns out to have been anticipated, inadequately perhaps but nevertheless explicitly, as the times were beginning to get ready for it. Thus the concept of a gradually changing *Zeitgeist* has been used to explain the historical continuity of thought and the observation that the novelty of a discovery, after the history of its anticipations has been worked out, appears often to be only a historian's artifact.

In addition to these anticipations there are, however, also the near-simultaneities and near-synchronisms that are clearly not plagiarisms. Napier and Briggs on logarithms. Leibnitz and Newton on the calculus. Boyle and Mariotte on the gas law. D'Alibard and Franklin on electricity. The sociologists Ogburn and Thomas have published a list of 148 contemporaneous but independent discoveries or inventions. Since you cannot in these pairs assume that one man got the crucial idea from the other, you are forced to assume that each had his novel insight independently by his ordinary processes of thought, except that each was doing his thinking in the same climate of opinion. Some such appeal to a maturing *Zeitgeist* is necessary to explain the coincidence (2).

Now how, we may ask, does the *Zeitgeist* of the present time interpret the generic concept of the *Zeitgeist*? Today the *Zeitgeist* is certainly *not* a superorganic soul, an immortal consciousness undergoing maturation with the centuries, an unextended substance interpenetrating the social structure. The *Zeitgeist* must be regarded simply as the sum total of social interaction as it is common to a particular period and a particular locale. One can say it is thought being affected by culture, and one would mean then that the thinking of every man is affected by the thinking of other men in so far as their thinking is communicated to him. Hence the importance of communication in science, which both helps and hinders progress. That is the thesis of this paper.

It is always hard to be original, to make progress

in a minority thinking that goes against the majority. In science, moreover, even the dead help to make up the majority, for they communicate by the printed word and by the transmitted conventions of thought. Thus the majority, living and dead, may slow up originality. On the other hand, the chief effect of scientific communication and of the availability of past thought is facilitative. We all know how the invention of printing advanced science.

We shall not be far wrong—being prejudiced, of course, by the *Zeitgeist* of the present—if we regard the scientist as a nervous system, influenced by what it reads and hears as well as by what it observes in nature and in the conduct of other men—the smile of approbation, the sneer of contempt—and affected also by its own past experience, for the scientist is forever instructing himself as he proceeds toward discovery and is also forever being instructed by other men, both living and dead.

The single investigator works pretty much like a rat in a maze—by insight, hypothesis, trial, and then error or success. I am not trying to say that rats are known to prefer deduction to induction because they use hypotheses in learning a maze. The maze is set up to require learning by trial and error, which is to say, by hypothesis and test. The rat's insight, as it learns, may indeed be false: the rat looks down the alley, sees it is not immediately blind but later finds it is blind after all. An error for the rat. And its trial may be vicarious. The rat looks tentatively down an alley, entertains it as a hypothesis, rejects it, chooses to go the other way. Anybody's hypothesis can come as the brilliant perception of an unexpected relationship and yet be wrong. It may be a hunch. Rodent hypotheses begin as hunches—and by this I mean merely that the rat does not understand the ground of its motives.

The human investigator, on the other hand, may consciously base his new hypothesis on his own earlier experiment, or on something other persons did. For this reason erudition is important, and communication is vital in modern science. Nevertheless it remains possible to regard the single scientist as an organic system, as a discovery machine, with a certain input from the literature and from other forms of social communication and also—let the essential empiricism of science not be forgotten—from nature, which comes through to insights and a conclusion by that method of concomitant variation which is experiment. There we have the individual investigator, who, as he grows older, gains in erudition and wisdom and becomes more mature, with his past discoveries now available as part of his knowledge.

A broader and more interesting question, how-

ever, concerns, not the individual, but the maturation of scientific thought itself. The mechanics of one person applies to too small a system to throw much light on the history of science. The larger view substitutes social interaction and communication for an individual's input, thus exposing the whole dynamic process as it undergoes maturation down the years, the centuries, and the ages. This interaction is the *Zeitgeist*, which is not unlike a stream. It is bounded on its sides by the limits of communication, but it goes on forever unless, of course, some great cataclysm, one that would make Hitler's effect on German science seem tiny and trivial, should some day stall it.

Here we have a physicalistic conception of the *Zeitgeist*. The *Zeitgeist*, of course, inevitably influences the conception of the *Zeitgeist*. And the *Zeitgeist* ought to be the property of psychologists, for the psychologists have a proprietary right in all the *Geister*. Now the psychology of the 19th century was dualistic, mentalistic, spiritualistic. In those days the *Zeitgeist* would certainly have had to be the maturing superconsciousness of science, something comparable to the immediate private experience that everyone then believed he had. The 20th century, on the other hand, at least since 1925, is physicalistic and behavioristic. Nowadays the term *behavioral sciences* is on everyone's lips and there is no English equivalent for *Geisteswissenschaften*.

Between 1910 and 1930 the *Zeitgeist* changed. Mind gave way to behavior. This transition was eased by the positivists who supplied the transformation equations from the old to the new, transformations by way of the operational definitions of experience; but only a few bother to use these equations. It is enough for most persons that they are using the convenient language of the great majority. And truth in science, as S. S. Stevens has pointed out, is simply what competent opinion at the time in question does not dissent from (3). In a physicalistic era, we, physicalistically minded scientists, choose a physicalistic definition of the *Zeitgeist*. Our predecessors in 1900 would not so easily have accepted such nonchalance toward Cartesian dualism.

We are wise thus to accept the wisdom of the age. Nor is my personal history without interest in this respect, for I was brought up in the introspective school of E. B. Titchener and for 20 years believed firmly in the existence of my own private immediate consciousness. Then, about 1930, en route to Damascus, as it were, I had a great insight. I knew that I was unconscious and never had been conscious in the sense that to have experience is to know instantaneously that you have it. Introspection always takes time, and the most

immediate conscious datum is, therefore, obtained retrospectively. Once this basic truth is assimilated, once one realizes that no system can include the report of itself and that to one's own introspection one's own consciousness is as much the consciousness of some "Other One" as is the consciousness of a different person, then it becomes clear that consciousness is not in any sense immediate, and then—just exactly then—the introspectionist gladly and sincerely joins the behavioristic school (4).

The *Zeitgeist*'s Dual Role

The *Zeitgeist* has a dual role in scientific progress, sometimes helping and sometimes hindering. There can be nothing surprising in such a statement. Forces in themselves are not good or bad. Their effects can be, depending on what it is you want. Inevitably by definition the *Zeitgeist* favors conventionality, but conventionality itself keeps developing under the constant pressures of discoveries and novel insights. So the *Zeitgeist* works against originality; but is not originality, one asks quite properly, a good thing, something that promotes scientific progress? In the cases of Copernicus, Galileo, Newton, and other comparably great men of science, originality was good—good for what posterity has called progress. These are the men to emulate. The indubitably original people are, however, the cranks, and close to them are the paranoid enthusiasts. Velikovsky's conception of the collision of two worlds is original. Does science advance under his stimulus? Hubbard's dianetics is original. Is it good? Most of us right now think not, yet these men point in self-defense to Galileo who also resisted the *Zeitgeist* (5). This dilemma arises because it is well to know and respect the wisdom of the ages and also to correct it when the evidence for change is adequate. If men were logical machines and evidence could be weighed in balances, we should not be mentioning the *Zeitgeist* at all. The *Zeitgeist* comes into consideration because it can on occasion work irrationally to distort the weight of the evidence.

When does the *Zeitgeist* help and when does it hinder the progress of science?

1) It is plain that knowledge helps research, and knowledge, whether it be explicit on the printed page of a handbook or implicit in the unrecognized premises of a theory, is in the *Zeitgeist*. There is no use trying to limit the *Zeitgeist* to that knowledge which you have without knowing it, for the line simply cannot be kept. One discovery leads to another, or one experiment leads to a theory that leads to another experiment, and the history of science tells the story. The law of multiple proportions, for instance, validates the atomic

theory, and then the atomic theory leads off to all sorts of chemical research and discovery.

On the other hand, the *Zeitgeist* does not always help, for there is bad knowledge as well as good, and it takes good knowledge to get science ahead. It is useful to be ignorant of bad knowledge.

The idea that white is a simple color was a bit of bad knowledge that was in the *Zeitgeist* in the middle of the 17th century. It was not a silly idea. It was empirically based. You can see colors, can you not? And white is a color. And you can see that it is simple and not a mixture, can you not? It is not clear whether Newton was lucky enough not to have absorbed this bit of false knowledge from the *Zeitgeist* or whether he was just stubborn, when, having bought his prism at the Stourbridge Fair, he concluded that white is a mixture of other colors. He was probably consciously flouting the *Zeitgeist*, for he sent his paper up to the Royal Society with the remark that it was in his "judgment the oddest if not the most considerable detection which hath hitherto been made into the operations of nature." But Robert Hooke and the others at the Royal Society would have none of it. They were restrained from belief by the *Zeitgeist*. White is obviously not colored, not a mixture. There was bitter controversy before the conventional scientists gave in, before the truth shifted over to Newton's side (6).

Helmholtz ran into a similar difficulty when in 1850 he measured the velocity of the nervous impulse. The *Zeitgeist* said: The soul is unitary; an act of will is not spread out over a period of time; you move your finger; you do not will first that the finger move with the finger not moving until the message gets to it. Thus Helmholtz' father had religious scruples against accepting his son's discovery. And Johannes Müller, then the dean of experimental physiology, doubted that the conduction times could be so slow. At the very least, he thought, the rate of the impulse must approximate the speed of light (7).

The persistence of the belief in phlogiston is still another example of the inertia that the *Zeitgeist* imposes on progress in thought. Here both Lavoisier and Priestley broke away from convention enough to discover oxygen, but Lavoisier, with the more negativistic temperament, made the greater break and came farther along toward the truth, whereas Priestley could not quite transcend his old habits of thought. His theory was a compromise, whereas we know now—insofar as we ever know truth in science—that that compromise was not the way to push science ahead then (8).

So it is. Good knowledge promotes progress, bad knowledge hinders, and both kinds make up the

Zeitgeist. Ignorance of good knowledge and awareness of bad hinder; awareness of good and ignorance of bad help. The history of science is full of instances of all four.

2) Not only do the discovery of fact and the invention of theory help progress when fact and theory are valid, but comparable principles apply to the discovery and invention of new scientific techniques. The telescope seems to have come out of the *Zeitgeist*, for it was invented independently by half a dozen different persons in 1608, although lenses had been made and used for magnification for at least 300 years. But then Galileo's discovery of Jupiter's moons the next year created, as it were, a new phase in the *Zeitgeist*, one that promoted astronomical discovery. So it was with the invention of the simple microscope, the compound microscope, the Voltaic pile, the galvanic battery, the galvanometer, the electromagnet, and recently the electron tube—the possibilities opened up by the availability of a new important instrument change the atmosphere within a field of science and lead quickly to a mass of valid research. Within psychology the experimental training of a rat in a maze in 1903, in order to measure its learning capacity, led at once to a long series of studies in the evolution of animal intelligence with the maze as the observational instrument.

It is true that the negative instances of this aspect of the *Zeitgeist* are not so frequent or obvious; yet they occur. For years the Galton whistle, used for the determination of the upper limit of hearing, was miscalibrated, because its second harmonic had been mistaken for its first. The highest audible pitch was thought to occur at about 40,000 cycles per second, whereas the correct figure is about 20,000. Did this error of an octave hold back science? Not much, but a little. For a couple of decades investigators reported facts about the octave above 20,000 cycles per second, an octave that is really inaudible. One experimenter even found a special vowel quality for it to resemble. Thus bad knowledge about the whistle led to confusion and hindered the advance of science.

3) The *Zeitgeist* acts as inertia in human thinking. It makes thought slow but also surer. As a rule scientific thinking does not suddenly depart widely from contemporary opinion. In civilization, as in the individual, the progress of thought is sensibly continuous. Consider, for example, the history of the theory of sensory quality.

Empedocles believed that eidola of objects are transmitted by the nerves to the mind so that it may perceive the objects by their images. Later there arose the notion that there are animal spirits in the nerves to conduct the eidola. Then, under

the influence of materialism, the animal spirits came to be regarded as a *vis viva* and presently a *vis nervosa*. Next Johannes Müller, seeing that every sensory nerve always produces its own quality, substituted for the *vis nervosa* five specific nerve energies, using the word *energy*, in the days before the theory of conservation of energy, as equivalent to *force* or *vis*. He said that the mind, being locked away in the skull, cannot perceive the objects themselves, or their images, but only the states of the nerves that the objects affect, and he fought a battle against the Empedoclean theory—as indeed had John Locke and Thomas Young and Charles Bell before him, and as still others were to do after him. After a while it was seen, however, that the specificity of the five kinds of nerves lies not in the peculiar energies that they conduct to the brain but in where they terminate in the brain. Thus there arose the concept of sensory centers in the cerebrum. Nowadays we see that a cerebral center is nothing more than a place where connections are made and that sensory quality must be understood in terms of the discriminatory response in which stimulation eventuates—or at least many of us see this fact while we fight a *Zeitgeist* that still supports the theory of centers (9).

Is there any reason why Galen in A.D. 180 or Albrecht von Haller in 1766 should not have invented the modern theory of sensory quality? None, except that most of the supporting evidence was lacking and, being contrary to the accepted notions of the time, it would have sounded silly. Yet each contributor to this strand of scientific maturation was original, and several contributors had to fight again the battle against the notion that the mind perceives an object by embracing it, or, if it cannot get at the object itself, by getting itself impressed by the object's eidolon or simulacrum. Nor has the *Zeitgeist* even yet been thoroughly disciplined in this affair, as you can tell whenever you hear the remark: "If the lens of the eye inverts the image of the external world on the retina, why do we not see upside down?"

The *Zeitgeist* was hindering progress in this piece of history. It made originality difficult and it made it necessary to repeat the same arguments in 1690 (John Locke) and in 1826 (Johannes Müller) and, if one may believe current advertisements of a scientific film, nowadays too. Yet let us remember that this *Zeitgeist* also helped progress. The continuity of development lay always within the *Zeitgeist*. It was a conservative force that demanded that originality remain responsible, that it be grounded on evidence and available knowledge. Had Galen espoused a connectionist's

view of sensory quality in the second century, he would have been irresponsibly original, a second-century crank, disloyal to the truth as it existed then. Loyalty may be prejudice and sometimes it may be wrong, but it is nevertheless the stuff of which responsible continuous effort is made. Science needs responsibility as well as freedom, and the *Zeitgeist* supports the one virtue even though it may impede the other.

4) What may be said of the big *Zeitgeist* may also be said of the little *Zeitgeister* of schools and of the leaders of schools and of the egoist who has no following. They have their inflexible attitudes and beliefs, their loyalties that are prejudices, and their prejudices that are loyalties. Every scientific in-group with strong faith in a theory or a method is a microcosm, mirroring the macrocosm which is the larger world of science.

Take egoism. Is it bad? It accounts for a large part of the drive that produces research, for the dogged persistence that is so often the necessary condition of scientific success. So egoism yields truth. It accounts also for the hyperbole and exaggeration of the investigating enthusiast, and then it may yield untruth. When two incompatible egoisms come together, they account for the wasted time of scientific warfare, for the dethronement of reason by rationalization. Egoism is both good and bad.

Take loyalty. Think how it cements a group together and promotes hard work. Yet such in-groups tend to shut themselves off from other out-groups, to build up their special vocabularies, and so, while strengthening their own drives, to lessen communication with the outside, the communication that advances science. Loyalty is both good and bad, and with loyalty a person sometimes has to choose whether he will eat his cake or will keep it.

This dilemma posed by the little *Zeitgeister* of the in-groups and the scientific evangelists has its root in basic psychological law. Attention to this is inevitably inattention to that. Enthusiasm is the friend of action but the enemy of wisdom. Science needs to be both concentrated and diffuse, both narrow and broad, both thorough and inclusive. The individual investigator solves this problem as best he can, each according to his own values, as to when to sell breadth in order to purchase depth and when to reverse the transaction. He, the individual, has limited funds and he has to sell in order to buy, and he may never know whether he made the best investment. But posterity will know, at least better than he, provided that it troubles to assess the matter at all, for posterity, having only to understand without hard labor, can assess the

effect of prejudice and loyalty and enthusiasm, of tolerance and intolerance, as no man ever can in himself.

Coda

This is a broad meaning for the word *Zeitgeist*—the total body of knowledge and opinion available at any time to a person living within a given culture. There is, certainly, no rigorous way of distinguishing between what is explicit to a scientist and what is implicit in the forms and patterns of communication, between what is clear conclusion and what is uncritically accepted premise. Available knowledge is communicated whenever it becomes effective, and this is the *Zeitgeist* working.

The *Zeitgeist* is a term from the language of dualism, while its definition is formally physicalistic. That paradox is for the sake of convenience in the present communication and is allowable because every statement can be transformed into physicalistic language when necessary. Dualism has the disadvantage of implying a mystery, the existence of a *Zeitgeist* as a vague supersoul pervading and controlling the immortal body of society. We need no such nonsense, even though this abstinence from mystery reduce us to so ordinary a concept as a *Zeitgeist* inclusive of all available knowledge that affects a thinker's thinking.

That such a *Zeitgeist* sometimes helps progress and sometimes hinders it should be clear by now. As a matter of fact, the distinction between help and hindrance can never be absolute but remains relative to some specific goal. The *Zeitgeist* hindered Copernicus, who, resisting it, helped scientific thought onward and presently changed the *Zeitgeist* on this matter to what it was in Newton's day. Did the *Zeitgeist* that Newton knew help relativity theory? No; relativity had to make its way against that *Zeitgeist*. The newest *Zeigeist*, which will include the principles of relativity and uncertainty and complementarity, presumably exists today within the in-group of theoretical physicists. It will become general eventually, and then it will reinforce progress, and after that, much later, perhaps our posterity will find today's truth tomorrow's error. The one sure thing is that science needs all the communication it can get. The harm communication does to progress never nearly equals the good.

References and Notes

1. The term *Zeitgeist* seems to have originated in this sense in 1827 with Goethe who, in discussing the way in which Homer had influenced thought, remarked in the last sentence of his essay, *Homer noch einmal*, "Und dies geschieht denn auch im *Zeitgeiste*, nicht verabredet noch überliefert, sondern *proprio motu*, der sich mehrfältig unter verschiedenen Himmelsstrichen hervortut." *Himmelsstrichen* can be translated "climates," thus justifying the figure of the text, but it must also be noted that Goethe meant to use the term *Zeitgeist* when the effect is "self-determined," brought about "neither by agreement nor fiat." See, for instance, *Goethes sämtliche Werke*, (I. J. Cotta, Berlin, 1902-07), vol. 38, p. 78.
2. The discussion of this paragraph and all the references will be found *in extenso* in E. G. Boring, "Great men and scientific progress," *Proc. Am. Phil. Soc.* 94, 339 (1950). The reference to Tolstoy is, of course, to his *War and Peace*. For the longest list of nearly simultaneous inventions and discoveries, see W. F. Ogburn and D. Thomas, "Are inventions inevitable?" *Polit. Sci. Quart.* 37, 83 (1922).
3. On the social criterion of truth, on scientific truth's being what scientists agree about, see S. S. Stevens, "The operational basis of psychology," *Am. J. Psychol.* 47, 323 (1935), especially p. 327; "The operational definition of psychological concepts," *Psychol. Rev.* 42, 517 (1935), especially p. 517; E. G. Boring, "The validation of scientific belief," *Proc. Am. Phil. Soc.* 96, 535 (1952), especially pp. 537 f.
4. On the point that a self cannot observe itself, that in self-observation a person must regard himself as if he were another person, see M. Meyer, *Psychology of the Other One* (Missouri Book Co., Columbia, Mo., 1921); Stevens, *op. cit.*, especially pp. 328 f.; E. G. Boring, "A history of introspection," *Psychol. Bull.* 50, 169 (1953), especially p. 183.
5. On the sincerity of cranks in science, see I. B. Cohen, J. L. Kennedy, C. Payne-Gaposchkin, T. M. Riddick, and E. G. Boring, "Some unorthodoxies of modern science," *Proc. Am. Phil. Soc.* 96, 505 (1952).
6. On Newton's difficulty in changing the *Zeitgeist* with respect to the complexity of white, see E. G. Boring, *Sensation and Perception in the History of Experimental Psychology* (Appleton-Century, New York, 1942), pp. 101 f. This discovery of Newton's was exceptional in that it had no anticipations (unless my wisdom is at fault). In other words, the *Zeitgeist* was strongly fixed, and to break it Newton must have been very stubborn—as indeed other evidence indicates that he was.
7. On Helmholtz' trouble with the *Zeitgeist* with respect to the velocity of the nervous impulse, see E. G. Boring, *A History of Experimental Psychology* 2 ed. (Appleton-Century-Crofts, New York, 1950), pp. 41 f., 47 f.
8. On Priestley and Lavoisier and the *Zeitgeist's* support of the phlogiston theory, see J. B. Conant, *The Overthrow of the Phlogiston Theory*, Harvard Case Histories in Experimental Science, Case 2 (Harvard Univ. Press, Cambridge, Mass., 1950).
9. On the history of the physiological theories of sensory quality and the retardation of progress in thinking by successive phases of this *Zeitgeist*, see E. G. Boring, *Sensation and Perception* (*op. cit.*), pp. 68-83, 93-95.



Influence of Philosophic Trends on the Formulation of Scientific Theories

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ACCORDING to Philipp Frank [*Sci. Monthly* 79, 139 (1954)], the reasons for, or against, the acceptance of certain scientific theories are not always restricted to the *technical value* of the theory in question—that is, to its ability to give us a consistent explanation of the phenomena it is dealing with; instead, these reasons involve a series of other factors.

Thus, in the case of Copernican astronomy, the choice was not merely one between a more simple and a more complicated theory of celestial motions, but also a choice between a more simple and a seemingly more complicated physics, between—as Bacon very aptly pointed out—reliance on, or rejection of, sense perception as a basis of physical knowledge, and so forth.

I am in perfect agreement with Frank. I only fear that he did not go far enough, and that, in his analysis, he made a rather unfortunate omission, namely, that of the philosophic background of the conflicting theories. It is, indeed, my contention that the role of this “philosophic background” has always been of utmost importance, and that, in history, the influence of philosophy upon science has been as important as the influence—which everybody admits—of science upon philosophy. As an example illustrating my assertion I would like to consider the period of post-Copernican science, the period commonly considered to be that of the *origin of modern science*, I mean the science that dominated our thinking for about three centuries, roughly speaking, from Galileo to Einstein and Planck.

Everybody agrees that the 17th century accomplished or underwent a deep cultural, philosophic, scientific revolution. From our point of view, and for our purposes, this revolution can be characterized as (i) the destruction of the *cosmos*, that is, the substitution for the hierarchically structured finite world of the Aristotelian tradition of the infinite universe bound together by the uniformity of its fundamental components and laws; and (ii) the geometrization of space, that is, substitution for the concrete physically structured place-space

of Aristotle of the abstract, isomorphous, and infinite dimension-space of Euclidean geometry now considered as real.

The cosmologic and physical conceptions of Aristotelian science, or natural philosophy, have—the existence of some orthodox Thomists notwithstanding—generally speaking a very bad reputation. Some of the moderns, especially philosophers and psychologists, go so far as to tax Aristotle’s reasonings as infantile and attribute to him the mental age of a child of 12.

Historians of science treat him scarcely better. This, in my opinion, is to be explained only by the continuity of the anti-Aristotelian tradition inherited from the founders of modern science who—and which—asserted themselves in a victorious struggle *against* Aristotle; and by the persistence of both the historiographic tradition of the 19th century and the value judgments of the first modern historians of science—an 18th and 19th century creation.

For these historians, born, bred, and reared in the Newtonian world, which—though with some rather important structural additions—has been accepted, not only as real and true, but even as conforming to the natural world-conception of the human mind, the very idea of a finite, closed cosmos appeared ludicrous. What ridicule, indeed, has not been piled upon Aristotle for his discussion of the dimensions of the world, or of its weight, or for holding that bodies could move naturally without being dragged or pushed from outside, for his belief that circular motion was a particularly interesting and important kind of motion, the very pattern of a natural one.

We know today—although, perhaps we have not yet quite *accepted* and *admitted* it—that all this is, perhaps, not quite so ridiculous as it seemed to be yesterday; and that Aristotle was much more right than he knew himself. As a matter of fact, circular motion *does* play a particularly important role in the world and is particularly well represented in it; so well that for a *natural* object it seems to be a *natural* thing to turn and rotate. Indeed, every-

thing does so: the galaxies and nebulae, the stars and the sun and the planets, and atoms and electrons; perhaps even photons are not an exception to the general rule.

The spontaneous motion of bodies, as we know full well since Einstein, is quite normal, provided that, of course, the space is conveniently curved; and we know, too, or at least believe we know, that our universe is by no means infinite (although it has no boundaries) and that "outside" this universe there is strictly nothing, just because there is no "outside" to the world, and all world-space is "inside."

Now this is precisely what—somewhat clumsily because he did not have at his disposal the resources of Riemannian geometry—Aristotle has been teaching us. Outside the world, he said, there is *nothing*, pure nothing, neither *plenum* nor *vacuum*, neither place nor space, because all the space—that is, all the places where something can be—is *inside*.

The Aristotelian conception is, of course, not mathematical; this is its weakness, but also its strength. It is a metaphysical one. The world of Aristotle is not a mathematically curved world; it is, so to speak, a metaphysically curved world.

Contemporary cosmologists, when they try to explain to us the structure of the Einsteinian, or post-Einsteinian, curved space and finite though boundless universe, are wont to point out that they are dealing with difficult mathematical conceptions, and that those of us who are not sufficiently trained and who lack ability in mathematical thinking will not be able fully to understand them. They are perfectly right, of course. Yet it is perhaps interesting to note that they are only repeating, indeed, turning upside down, what medieval philosophers, when they were dealing with Aristotelian cosmology, explained to their readers; thus they (Henricus of Ghent, for instance, in the late 13th century) did not fail to point out that they were using difficult metaphysical reasonings and concepts, and that those who were not sufficiently trained in, or gifted for, metaphysical thinking and who could not rise above the level of geometric imagining, could not understand Aristotle; they would continue to ask: What is outside the world? What will happen if we push a stick through the surface of the ultimate heavenly sphere?

The real difficulty of the Aristotelian conception—solved ultimately by Riemann and Einstein—consists, obviously, in the necessity of providing a place for Euclidean geometry inside the world. Yet, for Aristotle, it was by no means a decisive difficulty, for, in the Aristotelian conception, geometry is not a fundamental science that discloses the nec-

essary structure of physical being but only an abstract and subservient one. Experience, sense perception, not *a priori* mathematical reasoning, are for him the true bases of physics, the science that deals with nature and gives us knowledge of the real world.

On the other hand, for Plato, who believed in mathematics and did *not* believe in sense perception, and who had tried to link together the idea of a cosmos and an attempt to construct the physical world, the world of matter and of change, out of pure geometric space (*χώρα*), the situation was, of course, much more difficult. It had to be one or the other. The choice, sooner or later, had to be made. It was unavoidable, although about 2000 years passed before it was made in fact, and it was just this acceptance of the complete geometrization of space and, consequently, the rejection of the cosmos that characterized the Platonism of the 17th century, that of Galileo and of Descartes, and in this sense opposed it to the world-view of Plato himself.

It seems to me rather obvious that the revolution of the 17th century, which substituted for the qualitative world of sense perception and everyday life the Archimedean world of geometry made real, cannot be explained simply by the influence, or effect, of an enlarged or enriched experience (sense experience).

1) As P. Tannery and P. Duhem have already shown, Aristotelian science (physics), precisely because it was based on sense perception and common-sense experience and thus was really and truly *empirical*, was in much better accord with perception and experience than the new science of mathematical dynamics.

After all, bodies really fall when they are *heavy* and rise when they are *light*; and the principle of inertia, according to which bodies when pushed or thrown continue their motion indefinitely in a straight line, is certainly not based on experience, which constantly disproves the principle. Inertial motion, indeed, not only has never been encountered in the world, but it is even impossible that it ever should be.

2) The infinity of the universe cannot, of course, be asserted on the ground of experience. The infinite, as Aristotle had pointed out, cannot be traversed or given; compared with eternity a billion years is as nothing, and the world revealed by the Palomar reflector is by no means greater than that of the Greeks. Yet the infinity of the universe is an essential element of the axiomatic structure of modern science and is implied by its fundamental laws, as Euler and Einstein have both recognized.

3) The experiences alleged by the promoters of the new science, or their historians, prove nothing

whatever, because (i) such as they were actually made they are nothing less than precise, (ii) in order to serve as proof they would require an extrapolation to infinity, and (iii) they have, allegedly, demonstrated to us the existence of something—*inertial motion*—that is strictly and rigorously impossible.

The validity of these experiences presupposes the mathematical structure of nature, the mathematical language of (physical) science.

The birth of modern science is concomitant with a transformation (we could even call it mutation) of the axiomatic framework of human thoughts, with a shift in the evaluation of intellectual knowledge as compared with the knowledge given to us by sense perception, and with the discovery that—as Descartes suggested—the idea of the infinite is a clear and positive idea, in spite of its being (falsely) expressed by a negative term, and that it is *therefore* a true one—that is, an idea which gives us access to the real world.

Thus it is perfectly fitting that this infinitization of the universe (the breaking of the circle, as Marjorie Nicolson has called it, or the bursting of the sphere, as I prefer to call it) should have been announced by a philosopher—Giordano Bruno—and opposed for scientific and empirical reasons by the great astronomer Kepler.

Giordano Bruno is neither a very great philosopher nor a very good scientist, and the reasons he gives in favor of the infinity of the universe and the intellectual primacy of the infinite are not particularly clear. Giordano Bruno is not Descartes. Yet we know that not only in philosophy but even in pure science—take, for instance, the case of Kepler, or that of Dalton, or even that of Maxwell—faulty reasoning from inexact premises sometimes leads to perfectly sound and even extremely important results.

Fifteen years ago, I called the revolution of the 17th century “la revanche de Platon.” But, as a matter of fact, it was an alliance, an alliance with Democritus, that decided the old strife and enabled Plato to defeat Aristotle.

Strange alliance! Yet we know that not only in the history of philosophy or ideas but also in history *tout court* these strange alliances of seemingly, or even really, incompatible elements occur more often than not. The enemies of our enemies are our friends. Thus the Very Christian King of France allied himself with the Khalifa of Islam, Commander of the Faithful. Or to come back to the history of philosophico-scientific thought, what is more strange than the alliance of Mach and Einstein?

Democritean atoms in the space of Plato, or of Euclid: one understands that Newton needed a

God in order to maintain the connection between the elements of his universe; one understands, also, the rather curious character of this universe (the 19th century was too accustomed to it to be able to notice its strangeness) whose material elements, objects of a theoretical extrapolation of the experience, swim or are immersed, *without being affected by it*, in the nothingness of absolute space—a real and even necessary and eternal *non ens*—object of *a priori* intellectual knowledge. One understands therefore the rigorous mutual implication of this absolute, or of these absolutes—absolute space, absolute time, and absolute motion—that are accessible only to pure intellectual cognition, and their complementary opposites, relative space, relative time, relative motion that are the only ones given to us by empirical knowledge.

Modern science stands and falls with these conceptions of absolute time, absolute space, and their concomitants, absolute motion and absolute rest. Newton, as good a metaphysician as mathematician or physicist, recognized this perfectly well. And we can trust him in this case: only with these presuppositions and only on the basis of these fundamental assumptions are the *axioms* or *laws* of motion and of action valid and meaningful. The great Newtonians, MacLaurin and Euler, as well as the greatest of them, Laplace, fully recognized it too.

But let us come back to Newton. It is possible, according to him, that there is, perhaps, not one body in this our world that is really and truly at rest; moreover, even if there were one, we should not be able to recognize the fact and to distinguish it from a body in uniform motion. It is also true that we have no means, and can never have any, of determining the absolute motion of a body—its motion with respect to absolute space—but only and solely its relative motion, that is, its motion with respect to other bodies, about whose absolute motion we know no more than about that of the first. Yet these statements are not objections against these concepts; on the contrary, they are necessary and inevitable consequences of the very structure of their objects, that is, space and time. Moreover, Newton should not have been so prudent—although in the Newtonian world it is infinitely improbable that there ever should be a body at absolute rest and completely impossible that there should ever be one in uniform motion, Newtonian physics cannot avoid using these notions.

In the Newtonian world, and in Newtonian science—in spite of Kant who largely misinterpreted it but opened a way to a new epistemology and a new metaphysics, supporting a new science—the conditions of knowledge *do not* determine the conditions of being; quite the contrary, it is the struc-

ture of reality that determines which of our faculties of knowledge can possibly (or cannot) make it accessible to us.

Or, to use an old, Platonic formula: in the Newtonian world, and in Newtonian science, it is not man, but God who is the measure of things.

The interpretation of the history and structure of modern science that I have sketched here is not the *communis opinio doctrium*. At least not yet, though to judge by some recent publications, it is on the way to becoming just that.

Still, the prevailing trend of interpretation is somewhat different, and modern science is, as often as not, and even more often than not, presented as an example—the example—of empiricist or positivist epistemology. Historians and philosophers of the positivist school are wont to stress in the work of Galileo and of Newton (they usually pass over Descartes) their experimental aspect, presenting them as rejecting the search for *causes* and restricting the aim of science to the establishment of *laws*; not asking *why* something happens but *how* it happens.

This interpretation is certainly not lacking in historical basis; the role played by experience, or, more exactly, by experiment, in the history of science is more than obvious. Moreover, the works of Gilbert, Galileo, Robert Boyle, and others, are full of passages extolling the value and the fecundity of experimental methods and opposing them to the sterility of the speculative approach. And as for the restriction to the investigation of *laws* in preference to that of *causes*, everybody knows the famous passage from *Discourses and Mathematical Demonstrations concerning Two New Sciences* in which Galileo announces that it would be “unprofitable” for his purpose (which is, precisely, the establishment of the law of falling bodies) to discuss the explanations of gravity proposed and developed by his predecessors, just because nobody knows what gravity is—merely a word—and that, instead of trying to find out *why bodies fall*, it is much better to content ourselves with determining the mathematical law of their downward motion.

And everybody knows, too, the even more famous passages of the *Philosophiae Naturalis Principia Mathematica* (scholium) in which Newton says of gravity (which, meanwhile, has become universal gravitation) that hitherto he has not “been able to discover the cause of these properties of gravity from phenomena.” He writes:

I feign no hypotheses, for whatever is not deduced from the phenomena is to be called a hypothesis, and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical have no place in experimental philosophy. In this philosophy particular propositions are inferred

from the phenomena and afterwards rendered general by induction.

In other terms, relations established by experiments, or observation, are, by induction, transformed into laws.

Thus it is by no means surprising that, for a great number of historians and philosophers, this legalistic and phenomenalist aspect of modern science appeared to constitute its essence, or at least its *proprium*, in contradistinction to the deductive and realistic science of the Middle Ages and of Antiquity. Closely linked with it appeared also the pragmatic, active, technologic aspect of modern science—*scientia activa, operativa* of Bacon—as opposed to the allegedly contemplative, “theoretical” science of the past.

To this interpretation I have already made some objections. Two other objections are also pertinent.

1) Whereas the legalistic trend of modern science is indubitable, and besides has been extremely fruitful since it enabled the scientists of the 18th century to concentrate upon the study and analysis of the fundamental laws of the Newtonian universe, a work that culminated in the *Mécanique céleste* of Laplace and the *Mécanique analytique* of Lagrange, its phenomenalist aspect is much less apparent: as a matter of fact it is not the *phenomena*, but the *noumena* or the *noeta* that find themselves bound together by the causally unexplained or even unexplainable laws. Indeed, not bodies of our common-sense world, but abstract, Archimedian bodies of the Galilean one, or the particles and atoms of the Newtonian world, are the *relata* or the *fundamenta* of the mathematical relations established by modern science. Moreover, and this changes the picture somewhat, the law of attraction was transformed by the successors of Newton into a *cause* or *force*.

2) The positivist interpretation or self-interpretation of science is by no means “modern.” Quite the contrary: it is nearly as old as science itself, and like everything, or nearly everything, was invented by the Greeks, (as Schiaparelli and especially Duhem, after others, have quite convincingly established); the purpose of astronomical science, Alexandrine astronomers explained, is *not* to find out the real mechanism of planetary motion but only to *σώζειν τὰ φαινόμενα*, to save the phenomena by putting together a system of circles—a purely mathematical device—enabling us to calculate and predict the position of the planets, and thus establish a connection between the data of previous and future observations.

It is just this positivist-pragmatist epistemology that was used by Osiander (in 1543) in order to hide behind it the revolutionary impact of Copernican astronomy, and it was against this phenom-

enalist misinterpretation that was directed the emphasis of the founders of modern science from Kepler who puts ΑΙΤΙΟΛΟΓΕΤΟΣ in the very title of his work and calls it *Physica Coelestis*, to Newton who, in spite of his *hypotheses non fingo* that I have just quoted, gives us in the *Mathematical Principles of Natural Philosophy* not only a realistic, but also a causal science. He does this simply because, though he had renounced finding out the mechanical explanation of attraction and even rejected its reality as a physical force acting at a distance, he nevertheless posits it as a real, though nonmechanical and probably transphysical one, that subtends the mathematical "force" which binds the world together.

The real ancestor of positivist physics is by no means Newton. It is Malebranche.

Indeed, the Newtonian attitude concerning the problem of attraction is incompatible with the positivist point of view. For, from this point of view, there is no problem at all. Action at a distance, and even instantaneous action at a distance, which Newton so strongly opposed, is just as unobjectionable as any other kind of causation. E. Mach long ago, and P. W. Bridgman more recently and much more radically, have made it perfectly clear: to ask for continuity in space or in time is to be bound by prejudice.

As a matter of fact, this attitude has never been that of science. Action at a distance, even for those of the Newtonians who, following Cotes, accepted it as a physical force, has always been felt as something strange and difficult to admit—an absurdity to which one gets accustomed remains nevertheless an absurdity—and it is this conviction, which, by the way, could make an appeal to the

authority of Newton himself, that consciously inspired the work of Euler, of Faraday and Maxwell, and finally that of Einstein, who, by joining together the disconnected elements of the Newtonian world, space and matter, eventually solved the riddle.

So it seems to me that, in this case as in a number of others, it was not the positivist renunciation, nor the pressure of technical development of mathematical and experimental methods and procedures, but a philosophical attitude, that of *mathematical realism*, that has been the driving force or source of inspiration of the post-Newtonian development of scientific thought, the root of the concept of "field," that new key-concept of which Einstein has shown us the capital value for present-day science.

Thus I believe that we are entitled to conclude, tentatively at least, that (i) the positivistic phase of renouncement, or resignation, is only a kind of retreat position, and it is always a temporary one; (ii) although the human mind, in its pursuit of knowledge, repeatedly assumes this attitude, it does not accept it as final—at least it has never done so until now; and (iii) sooner or later it ceases to make a virtue of necessity and congratulate itself on its defeat. Sooner or later it comes back to the allegedly unprofitable, impossible, or meaningless task and tries to find a causal and real explanation of the accepted and established laws. And this, after all, is not surprising, at least for those who recognize that man is not only ζῶον ποιητικόν, a being of action, but also, and perhaps even primarily, ζῶον λογικόν, a being of reason, and by nature there is in man the desire not only to know but to understand.



Alternative Interpretations of the History of Science

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IN proposing several general questions on the social and historical aspects of science, I hope to state certain problems that have only too obvious a bearing on our work in the interpretation of science, to indicate certain alternative solu-

tions to these problems that have been proposed before, and to urge that certain of these are at best incomplete while the others are still merely plausible (1).

The achievement of understanding of the social

relations of science is a scientific problem that lies in the sciences of history and sociology. But it is not a problem solely in the history of science nor in the sociology of science even when the former is widened to include external (nonscientific) influences on the development of science. As in the solution of all scientific problems, this one involves data and theories. Thus the historicosociological events need to be established and while some primitive theory may be involved in the selection of epochs or regions to be investigated, yet there is a major fact-finding job for the student of the history of science. Utilizing these results, as well as those from other investigations, even from other sciences, the historian may be expected to formulate theories that explain how and why science developed including its mutually effective relations with the social (and sometimes physical and biological) environment. And we may expect some deliberate choice of specific historical investigations to lend credence to or to offer discrediting evidence about these explanatory interpretations.

I am neither a historian nor a sociologist, and at a symposium of the unity of science movement, I can only join with those who are deploring the extraordinary lack of detailed studies in the history of the social relations of science. I can only regret that the sociology of knowledge, and especially of science, has remained so long outside that movement's sweep, and so largely in the hands of metaphysically oriented phenomenologists and other speculative thinkers. The early death of Edgar Zilsel, a pioneer in the sociological treatment of science, left his work (2) tragically incomplete.

I

H. Guerlac has described an essential testing ground of certain theories about the development of science, namely the effect of political structure, and, *a fortiori*, of revolutionary changes in political structure, on science. E. C. Boring has considered the positive and negative influences of cultural milieu or *Zeitgeist* on the individual creation of scientific ideas and perhaps on activities of individuals and groups. And A. Koyré has focused this diffuse cultural influence upon that aspect which has been so very important in western European science, namely philosophic systems or philosophic outlooks.

I want to suggest what probably none of these writers would deny, that each article is part of a broader approach which relates the total aspects of society to science; and I shall offer a scheme which, because it is very simple, can relate the three articles and suggest certain problems and points of departure (3). Then I shall briefly apply this scheme to two particular scientific concepts.

The impacts of the sciences on the social order have perhaps been most often examined, and are known to take many forms, direct and indirect. I will, in view of this, turn my attention to the impact of the social order on science. In studying the role of the social environment, past and present, vis-a-vis science, it is just as well to state boldly what we want to know about the state of science in some detail, namely, why the following aspects exist as they do: (i) the social position of the scientific enterprise; (ii) the internal social characteristics of science, the variety and quantity of talent, the institutional forms, including professional societies and mediums of communications, forms of training and education of scientists, and of the public, and so forth; (iii) why certain problems are dealt with; (iv) why certain solutions (concepts and theories) are offered at the time they are offered; (v) why certain solutions are accepted; and (vi) why a mode of explanation is accepted, dominating the judgment of a man, a school or an epoch.

It is, of course, important to distinguish the sources of problems from the sources of answers and solutions to these problems. Likewise it is necessary to realize that we seek the historical genesis of these problems and of their solutions. Explanation of the history of scientific thought and practice will be genetic explanation (at least until historical explanation generally advances to now unknown characteristics).

The source of problems and solutions are of only two kinds, which may operate jointly in complex fashion and may be difficult to disentangle: (i) those due to previous stages of science, including of course other scientific activities, not logically connected to the particular scientific activity we seek to explain, and (ii) those due to nonscientific factors.

There is another way of typifying the sources: (i) those internal to the science, and that generate a problem logically, and (ii) those that pose problems only by external circumstance, in which case the neighboring sciences form part of the external group. Those who have explored the external factors have suggested a wide variety of social influences on science: the influence of religion—Max Weber, R. K. Merton, Charles Raven, G. N. Clark, and S. F. Mason (4); the influence of art—L. L. Whyte, Herbert Read, and occasionally A. N. Whitehead (5); the effect of social institutions—Thorstein Veblen, Max Weber, and Lewis Mumford (6); the influence of philosophy—Plato, J. S. Haldane, J. Maritain, A. Koyré, E. Meyerson, J. Nef, and F. S. C. Northrop (7); the influence of the economic order and its cultural and ideologic reflections—Karl Marx (8); the distor-

tion of political institutions and misconceptions of other sciences: K. Popper, M. Polanyi, F. Hayek (9); the influence of social images projected into nature—H. Kelsen (10); the influence of irrational choices of occupation and content—S. Freud (11).

All of these have been documented sufficiently and we may now ask that some comparative analysis be made. The significant hypothesis today, and for some time past, would be one which tries to determine the relations of dominance—for a specific period of science, or even a specific scientific event—among the many factors that have been suggested. The sweeping views of a Whitehead that the bifurcation of mind-body can be traced to Cartesian philosophy; or of Northrop that the science and general culture can be determined by an epistemological decision about the apprehension of nature; or of Freud's irrational determinants, or of the others, need to be put to a test which is somewhat different from that which they themselves offer. By this I mean that their exposition seems to be incomplete.

We must ask Koyré and Whitehead why a new philosophy of the cosmos, of a science that uses material, unspiritual, and nonmental substance, gains dominance? Why and where does it arise? We must ask Northrop, why should an epistemological policy be proposed in the first place—surely not because of a previous epistemological decision—and why should it have been accepted as orthodox or normal in the second place? Or, would Northrop claim that the history of epistemology has an independent development from which the history of culture, and indeed general history, is derivative?

To put these questions more generally, we must ask why the undoubted relations I have just listed between science and the many aspects of social and individual life, exist as they do. Thus the study of the history of science must be carried on as part of the study of the history of general culture. For example, the study of the influence of religion on science—productive or restrictive—is only a step toward the study of the history of religion, not in a mere descriptive sense as a sequence of stages that no doubt influence science, but as part of cultural history in its own right, with its own demand for knowing why the religious forms developed as they have. Only if the history of religion is shown to be independent, or else dependent on still another aspect of society, will the religious explanation of scientific endeavors approach completeness. (Of course, if the history of religion is not independent and self-generating, then we must ask the same question of the historian of the sources of religion, in his turn.)

II

In the history of science there have been problems which, looked at logically and with hindsight, demanded treatment and yet were not considered by scientists; or, if they were considered, no headway was made either in theoretical treatment or in accumulation of evidence; or, again, that theory which satisfied an earlier scientific public seemed to a later scientist only to add to the puzzle (12). As often as not the later examination with changed criterions provided the stimulus to more comprehensive theory and more thorough research. Contrary to the thesis of Boring, I would say that men always go with the *Zeitgeist*; when they seem to oppose it, they do so within it, not against it, thereby revealing contradictory tendencies within it. We, as self-conscious and society-conscious thinkers, recognize that the sources of creative scientific theory—the ideas, analyses, visions of models, formal systems, experimental devices, and so forth—may exist in any aspect of human experience whatsoever (13); and we are receptive to the notion that all human events, even creative acts, have their genesis in other ascertainable parts of human culture.

How can we protect scientific knowledge from its varied historical origins and influences? We may be tempted to use a principle of verification, and with it to recognize the cumulative nature and logical rigor of science. But I doubt that the verification principle taken by itself is sufficient to shield science. If we disregard the question, "Should empirical practice serving as verification be the filter for distinguishing true from false ideas?" we must recognize that (i) only those truths (and falsehoods) can be filtered which a given environment provides (including internal as well as external factors), and error by omission and distortion may be great, (ii) the standards for judging truth are social products, and hence Boring's *Zeitgeist* is a product of the *Zeitgeist*, (iii) practice has not, in fact, been the test for all the accepted truths of Western science, not even some contemporary science.

We have, then, to distinguish three stages in the social influencing of scientific ideas: (i) the problem that is attacked, (ii) the ideas and techniques brought to bear on it, and (iii) the principles of verification that the society, as part of its culture, provides. To free our science from its own socio-centric predicament would require as much attention to the third, sociology of epistemology, as to the distortions introduced by the second, and the sins of omission in the first. And even then it is only a hypothesis that such self-knowledge would release a greater science than the past, in the sense that for all we know knowledge, even self-knowl-

edge, may be insufficient and unnecessary for the production of knowledge.

My own feeling is that social conditions external to science act in two ways. In the first place, they are obviously sufficient to pose problems and direct the course of science or frustrate it utterly. In the second place, what is less evident, they are necessary for solving problems as sources both of ideas and of the verification principle. I shall sketch two examples briefly.

III

The development of the first two laws of thermodynamics came long after the scientific data were available, and they arose because of the social stimulation of steam power engineering on three men who were not fixed in their researches and thinking by the orthodox physics and chemistry of their day (14). Carnot was deliberately and explicitly seeking to improve the efficiency of the steam engine when he formulated the second law in 1824. Joule, in reaching the first law, correlated electric energy with mechanical work and then with heat in an attempt to make an electric engine that would be more efficient than a steam engine; and by 1843 the juggling of these ideas into a mechanical equivalent of heat had occurred. Mayer, as a ship's doctor, found in 1841 a need to suppose a mechanical equivalent of heat in discussing vital processes akin to combustion. He used Lavoisier's theory of physiological combustion, and quite the same data as Lavoisier had used 60 years before. But, as Lilley puts the matter so well, Mayer viewed the body as a heat engine, Lavoisier viewed it as a furnace or fire. Mayer even used the steam locomotive analogy as a persuasive argument for the reader of his first paper.

All these early discoverers of the conservation of energy, Carnot, Mayer, and Joule, argued that *vis viva*, (or twice our kinetic energy) was too important to be destroyed, quite like advocates in earlier centuries had argued about material substance. The same argument was not offered by Rumford in 1798, yet it seemed so natural in 1840 as itself to need no support. The cultural climate was so sharply different that Rumford, while discussing the generation of heat by mechanical friction never seems to have thought about the converse effect, namely the generation of mechanical motion by heat, even though the steam engine was already in some use and even though he was active in applying science to practical affairs. It was the wide growth of engineering research by nonscientists and the change in the *Zeitgeist* which made possible the new explanation. Actually the explanation was couched in almost metaphysical language and in highly *a priori* terms; this is surely a sign

that it was in the cultural air rather than actually *a priori*. It is notorious that the data did not prove the hypothesis of conservation of energy. The view that they did was based on arguments that would horrify an inductive statistician. The ways in which some scientific hypotheses and principles of high order and great importance have been brought to science and accepted by scientists should make a logical analyst shudder. The role of dogmatic *a priori* and of analogies and of new problems disconnected from previous science has often been all-important.

IV

The concept of laws of nature likewise exhibits social origins, both as problem and as hypothetical solution of the problem (15). The problem seems as old as magic and ever-present, namely why is the world so irregular, willful, and unpredictable? Or, are there regularities behind the veil of apparent irregularities and dissimilarities? If so, why the regularities? Now law, as a supreme legislation for the behavior of natural objects, arose in the ancient world along with a society that had just become centralized and capable of its own universal legislation, namely the joint appearance of the Babylonian creation myth of Marduk and the Hammurabi legal code in 2000 B.C.

The pre-Socratics and Plato and Aristotle almost never used the idea of a law of nature (though they frequently used the notion of unlegislated and uncreated "natural" necessities or characteristics. By contrast they do have laws of men and gods). But the Stoics reintroduced law of nature again about 200 B.C., again at a time of political centralization in the new great Hellenistic monarchies. It was only plausible that Roman Stoics would continue this conception in the Roman empire, composed of many cultures and one law. Indeed the idea of an empirical natural law for all men and laws of nature for all nature was, broadly, a Roman commonplace. (Again the supreme lawgiver of ancient Judaism and later Judeo-Christian ethical religion helped to give science its idea of law.) Note that the naturalistic Democriteans and Epicurus do not speak of laws of nature, for they have freed themselves from the social image of a creator. In all these, the laws of man and of nature are not separated. Indeed medieval writers often speak of animals as obeying a code laid down by God and of men as obligated to obey their human rulers only if the man-made laws conform with God's.

In modern science, the natural law as metaphor became explicitly so by the 17th century (and distinguished from human and moral laws of behavior of man), the first separation into metaphor

occurring about the time of Kepler. Descartes speaks of the laws which God put into nature, Spinoza of the metaphor of Descartes, for Spinoza's pantheistic God could not easily be a lawgiver. Note that God, in earlier medieval time, was the divine will that brought the exceptions to the world, the irregular occurrences, for example comets and monsters. Now, after Descartes He is the lawgiver for the regular occurrences. Again the social model is the rise of a centralized political and legal authority, the new centralized nation-monarchies, the decline of local nobilities. As Zilsel pointed out, Descartes' idea of God as universal legislator occurred soon after Jean Bodin's new theory of sovereignty. The coincidence deserves careful examination from the historian of ideas.

The conception of an imposed law for nature originated in Oriental absolutism, declined in the Greek political fragmentation, flourished in the Greek and Roman world states, languished in medieval politically decentralized feudalism, (despite religious centralization), and revived with the birth of capitalist international relations and centralized nation states, attaining a metaphysical status in its new naturalistic revival. Of course this quasitheological stage of science has now been abandoned, an event dramatized by Laplace's abandonment of the hypothesis of a universal legislator. We have now, in Pearson's phrase, descriptions instead of prescriptions. But could the recognition of statistical regularities and their mathematical expression have been reached by any other road than the theological one we actually traveled?

The only test comparison with a developed civilization is that of nontheological China. As Needham and Northrop have remarked, theology in China has been so depersonalized, law made so ethical, humanistic, and particular, that the idea of a rational creator of all things was not formulated. Hence the idea that we lesser rational beings might, by virtue of that Godlike rationality, be able to decipher the laws of nature (in Galileo's phrase, we might read the mathematical language of the Book of Nature) never was accepted.

The *Zeitgeist* of China exhibited a doctrine of harmony and of wholes, behavior in the natural expression of inner drives, not the expressed action of eternal and superior authority. China could have had a philosophy of organism, and if the other societal factors had given rise to a great activity aimed at understanding and controlling detailed natural processes (that is, science), it would have been functional at its base and probably nonmechanical. But if such scientific activity had been stimulated theology might have developed too! And who can tell whether the theological mecha-

nism was not really necessary to have naturalistic science, or mechanical science to have a science of process and organism? As Needham asked "Was the state of mind in which an egg-laying cock could be prosecuted at law necessary in a culture which should later have the property of producing a Kepler?"

References and Notes

1. I am indebted to the Faculty Research Committee of Wesleyan University for grants which helped make possible the studies in the history and philosophy of science on which this article is based. A portion of this article was read as part of a paper presented to the Connecticut Section of the History of Science Society, 30 Oct. 1953. An expanded version of that paper will appear in the publications of the Section under the title "Galileo and Mayer: Two studies in metaphysics and mechanics".
2. It would be helpful if Zilsel's essays were brought together in one volume, but as yet no publisher has been found for such a project. The papers readily available in this country include: "History and biological evolution," *Phil. Sci.* 7, 121 (1940); "Copernicus and mechanics," *J. Hist. Ideas* 1 113 (1940); "The origins of William Gilbert's scientific method," *ibid.* 2, 1 (1941); "Phenomenology and natural science," *Phil. Sci.* 8, 26 (1941); "Physics and the problem of historico-sociological laws," *ibid.* 8, 567 (1941); "Development of empiricism," *International Encycl. Unified Science* II 8, 53 (Univ. of Chicago Press, 1941); "The genesis of the concept of the physical law" *Phil. Rev.* 51, 245 (1942); "The sociological roots of science" *Am. J. Sociol.* 47, 544 (1942); and "The genesis of the concept of scientific progress" *J. Hist. Ideas* 6 325, (1945).
3. While writing these lecture notes for publication, two recent books came to my attention which are of direct relevance to the interpretation of science. Some of the issues I have raised are treated at greater length in them, and if this paper can call them to the attention of American scientists it will have been successful. One is the special issue, "Essays on the Social History of Science," of the Danish journal *Centaurus* 3, 1-2. Of direct relevance here are the essays by Childe ("Science in preliterate and the ancient Oriental civilizations"), Farrington ("The rise of abstract science among the Greeks"), Needham ("Thoughts on the social relations of science and technology in China"), Lilley ("Cause and effect in the history of science"), Taton ("The French revolution and the progress of science"), and Forbes ("Metallurgy and technology in the Middle Ages"). The second is J. D. Bernal, *Science and Industry in the Nineteenth Century* (Routledge and Kegan Paul, London, 1953), which contains a long title essay and a shorter acute discussion "Molecular asymmetry" given as homage to Pasteur. Bernal's book is noteworthy for its attempt at uncovering different types of technologic influence on the several sciences, and at different times in the course of the rise of the industrial era, and at relating something of the reciprocal nature of these influences. In addition, several other works are promised that will shed light on these matters: *A History of Technology*, C. Singer, E. J. Holmyard, and A. R. Hall, Eds., to appear in 5 vols., of which vol. 1, *From Early Times to the Fall of Ancient Empires*, has been published (Oxford Univ. Press, 1954); J. D. Bernal, *Science in History* (Watts, London, 1954), one chapter of which appeared as "The place and task of science," *Science and Society* 13, 193 (1949); and J. Needham, *Science and Civilization in China*, to appear in 7 vols., of which vol. 1, *Introductory Orientations*, has been published

- (Cambridge Univ. Press, Cambridge, 1954), and of which vol. 2 will be entitled *History of Scientific Thought*; and *Science, Medicine and History: Essays in honor of Charles Singer*, 2 vol. (Oxford Univ. Press, Oxford, 1954).
4. M. Weber, *The Protestant Ethic and the Spirit of Capitalism*, tr. Parsons (Allen and Unwin, London, 1948) especially note 145; R. K. Merton, "Puritanism, pietism and science" *Social. Rev.* 28, 1 (1936) reprinted in his *Social Theory and Social Structure* (Free Press, Glencoe; Ill., 1949), and "Science, technology and society in 17th century England" *Osiris* 4 part 2 (Bruges, Belgium, 1938); J. Needham, "Science, religion and socialism" and "Pure science and the idea of the holy" in his selected essays *Time, the Refreshing River* (Allen and Unwin, London, 1943); C. Raven, *Science and Religion* (Cambridge Univ. Press, Cambridge, 1952); S. F. Mason, "Science and religion in seventeenth century England" *Past and Present* 1 No. 3, 28 (1953).
 5. L. L. Whyte, Ed, *Aspects of Form* (Lund Humphries and the Institute of Contemporary Arts, London, 1951); H. Read, *Art and Society*, rev. ed. (Faber and Faber, London, 1945); A. N. Whitehead, *Adventures of Ideas* (Macmillan, New York, 1943). M. Johnson, *Art and Scientific Thought* (Faber and Faber, London, 1946); W. M. Ivins, *Art and Geometry* (Cambridge: Harvard Univ. Press, 1946).
 6. T. Veblen, *The Place of Science in Modern Civilization and Other Essays* (Viking Press, New York, 1919), and also J. Dorfman, *Thorstein Veblen and His America* (Viking Press, New York, 1934); M. Weber, *op cit.* (4), and also see his *From Max Weber: Essays in Sociology* (Oxford Univ. Press, New York, 1946); Lewis Mumford, *Technics and Civilization* (Harcourt, Brace, New York, 1934).
 7. J. S. Haldane, *The Sciences and Philosophy* (Macmillan, London, 1926); J. Maritain, *Science and Wisdom* (Bles, London, 1944); E. Meyerson, *Identity and Reality* (Allen and Unwin, London, 1930); E. A. Burtt, *Metaphysical Foundations of Modern Physical Science*, rev. ed. (Routledge and Kegan Paul, London, 1932); F. S. C. Northrop, *The Meeting of East and West* (Macmillan, New York 1946), and "The criterion of the good state," ch. 17 of *The Logic of the Sciences and the Humanities* (Macmillan, New York, 1947); the influence of general philosophical values turns out to be the theme of an essay little noticed by scientists, and which is initially devoted to the much discussed question of the coincidental rise of science and industrialism, J. U. Nef, "The genesis of industrialism and of modern science (1560-1640)" in *Essays in Honor of Conyers Read* (Univ. of Chicago Press, Chicago, 1953).
 8. Karl Marx and Friedrich Engels, *The German Ideology* and also passages in Marx's *Capital*, vol. I International Pub., New York, 1939; J. D. Bernal, *The Social Function of Science* (Routledge, London, 1939), and *Marx and Science* (Lawrence and Wishart, London, 1952); B. Hessen, "The social and economic roots of Newton's 'Principia'" in *Science at the Crossroads* (Kniga, London, 1931); A. R. Hall, *Ballistics in the Seventeenth Century* (Cambridge Univ. Press, Cambridge, 1952), a careful examination of the relations of science and war technology, which tends to confirm Bernal's cautious formulations of the Marxist thesis while casting doubt on the cruder work of Hessen, G. N. Clark, *Science and Social Welfare in the Age of Newton*, ed. 2 (Oxford Univ. Press, Oxford, 1949), a critical study of the Marxist thesis. It should, perhaps, be noted that the Marxist theory of scientific knowledge does not relegate all knowledge in capitalist or earlier societies to some ideological limbo of illusion, although the theory does imply that all knowledge is obtained from the perspective of the society which obtains it, and hence is distorted appropriately. It is an active research problem in Marxist sociology to determine the criterions for distortions and illusions as contrasted with the objective truths in a historically class-sponsored system of knowledge. The general view is that facts and mathematical formulas (to use the science of physics) are objective, but that the interpretation of the symbols tends to be ideologically distorted, for example the formulas of quantum mechanics as contrasted with the Bohr interpretation. For a popular provocative essay on this, see C. Caudwell, *The Crisis in Physics* (Bodley Head, London, 1939).
 9. K. R. Popper, *The Open Society and its Enemies* (Routledge, London, 1945); M. Polanyi, *The Logic of Liberty* (Routledge and Kegan Paul, London, 1951); F. A. Hayek, *The Counter-Revolution of Science*, (Free Press Glencoe, Ill., 1952).
 10. H. Kelsen, *Society and Nature* (Kegan Paul, London, 1946).
 11. S. Freud, *Civilization and Its Discontents* (Hogarth Press, London, 1949), for example, p. 63, also his *The Future of an Illusion* (Hogarth Press, London, 1943) especially the concluding sections, and finally his *New Introductory Lectures on Psycho-Analysis* (Hogarth Press, London, 1949), especially Lecture 35.
 12. See the chapter "The postponed scientific revolution in chemistry" in H. Butterfield, *The Origins of Modern Science 1300-1800* (Bell, London, 1949 and Macmillan, New York, 1950), and, for studies of scientific prophets before the times, see, in physics for example, D. W. Singer, *Giordano Bruno His Life and Thought* (Schuman, New York, 1950) and in history, H. P. Adams, *The Life and Writings of Giambattista Vico* (Allen and Unwin, London, 1938). The converse effect, namely very advanced technical accomplishments that could not be utilized until social demands reached a certain level of technologic competence, is discussed in A. R. Hall, *op. cit.* (8).
 13. See, for example, J. Hadamard, *An Essay on the Psychology of Invention in the Mathematical Field* (Princeton Univ. Press, Princeton, 1949).
 14. Although I was led to the thought of this section by reading Mayer, it was gratifying to find the same conception in the exploratory article by S. Lilley, "Social aspects of the history of Science," sect. vi, *Arch. intern. hist. sci.* 28, 376 (1949).
 15. This section is broadly due to the researches of Zilsel. See also Kelsen, *op cit.* (10); E. V. Arnold, *Roman Stoicism* (Cambridge Univ. Press, Cambridge, 1911); and J. Needham's Hobhouse memorial lecture for 1950, *Human Law and the Laws of Nature in China and the West* (Oxford Univ. Press, London, 1951) from which the final quotation is taken (p. 42). Needham's studies are carried further in his article "The pattern of nature-mysticism and empiricism in the philosophy of science third century B.C. China, tenth century A.D. Arabia, and seventeenth century A.D. Europe" in the second volume of *Science, Medicine and History* (3). My remark about the organic self-causation views of Chinese thought are due, in part, to Derk Bodde "Harmony and Conflict in Chinese Philosophy," an essay in *Studies in Chinese Thought*, Arthur Wright, Ed., Memoir 75 of the American Anthropological Association (Chicago, 1953).

Philippine Hydroelectric Development

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IN October 1953 the Philippine Republic celebrated the completion of the first phase of the Maria Cristina power project in northern Mindanao. Maria Cristina symbolically portrays the interest of the Philippines in developing a power base for industrial utilization of Philippine resources. Because the islands possess little coal and no known petroleum deposits of significance, hydroelectricity is of necessity a logical and economically practical source of power for the development of industry and the improvement of living conditions. In addition, the mountainous terrain and the heavy annual rainfall make hydroelectric projects obvious possibilities.

Power development in the Philippines is entrusted to the National Power Corporation, an instrument of the Philippine Republic. Organized in 1937, the corporation started its first hydroelectric project on the Caliraya River southeast of Manila in 1939. Although the plant was completed by the Japanese in 1942, it was dynamited during the battle of liberation. Rehabilitation was completed in October 1947. The Caliraya project is on a plateau more than 1000 ft above sea level and includes a rolled earth dam across the river. During the rainy season the dam impounds water in a reservoir that has a useful storage capacity of 78 million cubic meters. Three 60-cy/sec generators, each rated at 10,000 kva, were originally installed (1).

Postwar plans. After the liberation, the National Power Corporation, in collaboration with Westinghouse Electric Company of the United States, established a postwar rehabilitation and long-range development plan consisting essentially of four main parts.

The first project in the first part of the long-range program was the construction of another rolled earth dam across the Lumot River, a tributary of the Caliraya. The resulting reservoir pro-

vided water for diversion to the Caliraya project and thus made possible the use of a fourth 10,000-kva generator. Completed in 1951, the combined Caliraya-Lumot project produces 38,000 kw, 36,000 of which are sold to the Manila Electric Company for distribution in Manila (2). Although unrelated to the Caliraya-Lumot project, the Ambuklao and Binga projects on the Agno River in northern Luzon are also included in this part of the program.

The second part of the development program involves the construction of the Maria Cristina power-industrial project on the Agus River in northern Mindanao.

The construction of a number of small hydroelectric projects scattered over the country, with a total capacity of 8000 kw, constitutes the third part, and the construction of steam plants with a total capacity of 5000 kw in areas without water-power sites represents the fourth part of the program.

At present the Caliraya-Lumot project, as well as several small scattered ones, is completed. Work is progressing on the Ambuklao site, and the first phase of the Maria Cristina project is in operation.

Ambuklao-Binga projects. A major aspect of Philippine power development is a multiple-purpose project in which the Agno River of northern Luzon is to be harnessed for power production, flood control, and irrigation (3). The Agno River heads on the southern slopes of Mount Data, approximately 70 km northeast of Baguio in Mountain Province, Luzon. Flowing southward for about 110 km between two great ranges of the Central Cordillera mountain chain, the river debouches into the central plain of Luzon at San Manuel, Pangasinan. Following a semicircular route to the west across a flat alluvial plain, the river empties at last into Lingayen Gulf, an arm of the China Sea.

During the early Cenozoic age, a series of lava flows and water-deposited sedimentary rocks were laid down in this region. Later, mountain-building forces fractured and metamorphosed the lavas and sediments, and intrusions of molten diorite took place.

After their formation, the rocks were long subjected to erosion, during which time the Agno Valley was eroded nearly to sea level one or more times. In addition the river was in a stage of aggradation for a period, and it developed a meandering course not controlled by the structure of underlying rock. Recent uplift has brought renewed downcutting, causing the stream to intrench itself. Wherever the river was by accident superimposed on a zone of weakness, it has straightened its course, so that in some places it seems to be subject to structural control, but not in others.

Today the watershed is geologically young, very mountainous, and subject to rapid erosion because of the steep hillsides and perpendicular precipices along the canyon. The valley itself is narrow with few suitable reservoir sites. In a distance of 50 mi above its exit from the mountains, the river drops approximately 2000 ft. During the July-to-December rainy season typhoon rains cause heavy floods, but during the dry season flow drops to as little as a few cubic meters per second. The watershed has received what is perhaps a world's record rainfall for a single storm, with 48 in. in 24 hr and 105 in. in less than a week (Figs. 1, 2).



Fig. 1. Central Cordillera of north-central Luzon. A tributary valley illustrates the rugged topography typical of the Agno Valley. Primitive Kaingin (shifting) cultivation by Igorot farmers has destroyed the forest cover. Scene near Itogon gold mine.

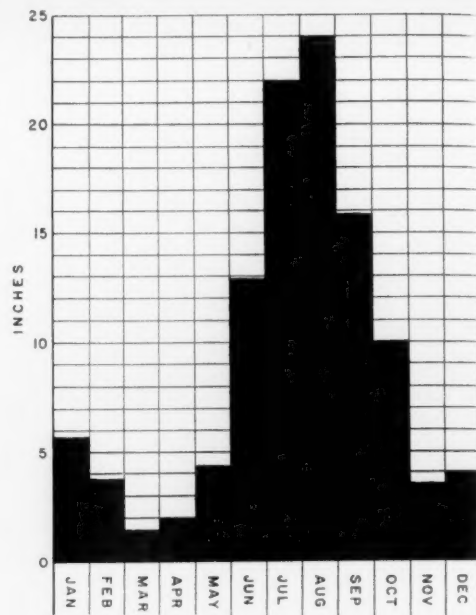


Fig. 2. Rainfall distribution in the Agno River-Bagui area points out the need for both flood-control and irrigation aspects of the Agno project. The wet season is associated with southwest monsoon, but floods are caused primarily by typhoons. [Source: Philippine Air Lines. Data from Philippine Weather Bureau]

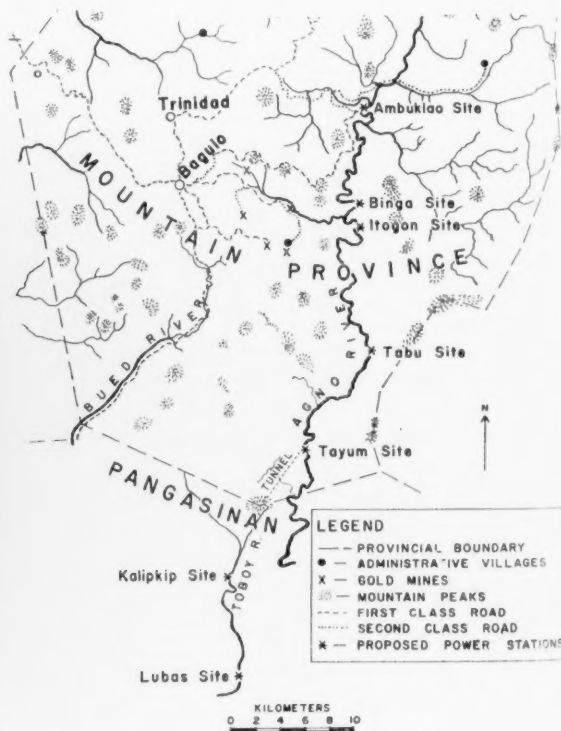


Fig. 3. Seven actual power sites will eventually be constructed on the Agno-Toboy rivers. Water from the Agno will be diverted by tunnel to the Toboy for further control and power development.

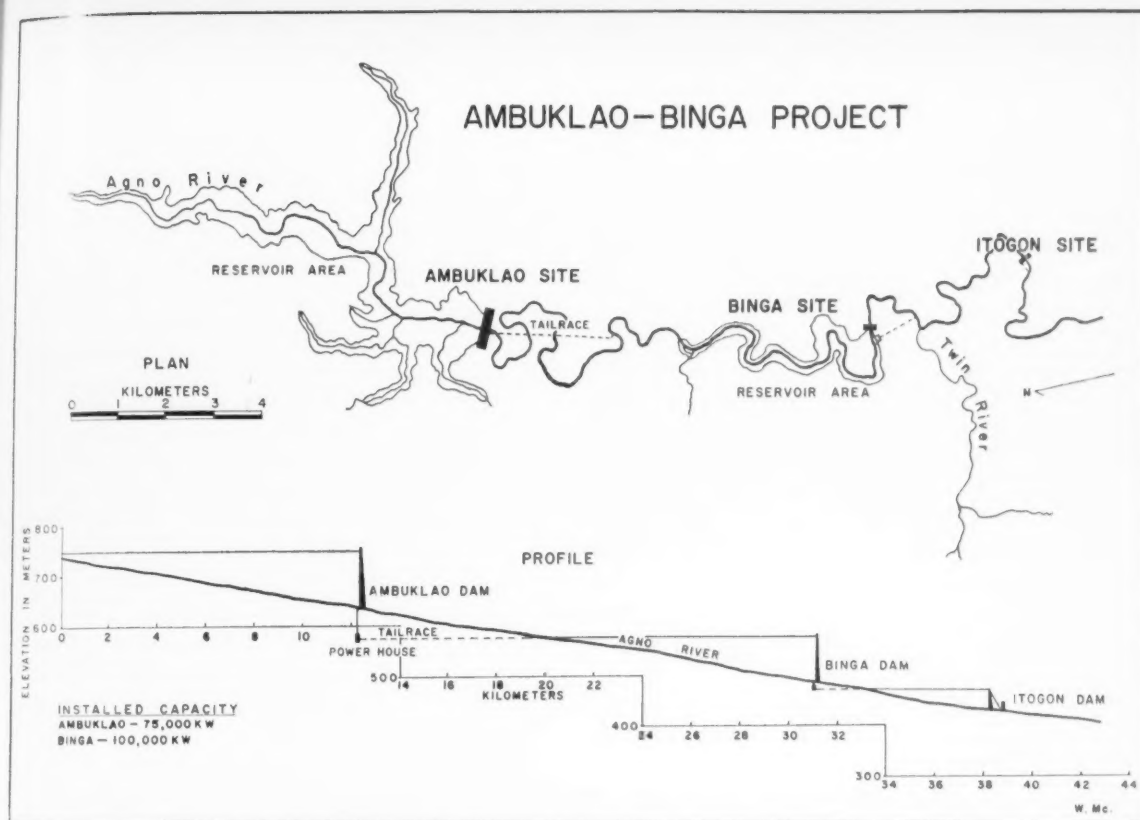


Fig. 4. Ambuklao Dam, with its large reservoir, will feed lower stations as well as generate power at its own site. Binga, with a much smaller reservoir, will provide peaking capacity for Ambuklao, making the two essentially one project.

As a result of its geologic and physiographic history, the Agno River represents a considerable potential of hydroelectricity, but there are many difficulties to be faced in actually producing it. Although the diorite and metamorphic rocks are deeply weathered at the surface, as is usual in the tropics, the rock below is very hard, forming adequate foundations. With care, the fractured zones can be avoided in subsurface construction. By providing relatively high dams for hydroelectric development, some measure of regulation of flow can be attained and minimum flow can be increased to a discharge level high enough to permit the use of water throughout the year for power and irrigation.

Three reservoir sites have been chosen on the Agno River and others on the Toboy River, a tributary to which water from the Agno is to be diverted for further regulation and power development (Fig. 3). The uppermost of the reservoirs is to be created by the Ambuklao Dam. At this site the river flows from north to south in a narrow gorge between two ridges. Above the gorge the river has a relatively wide valley for the reservoir basin. Below

the gorge the river flows in a narrow meandering valley with several marked loops in the course just below the dam. By running the tailrace tunnel under several of these loops, it is possible to obtain a greater head than if the power plant emptied directly into the river at the power site.

Since Ambuklao has the largest reservoir capacity and is uppermost on the river, its water will be drawn upon to the practical limit for use in several downstream projects. Because of the large possible drawdown from a net head of 564 to 380 ft, Ambuklao is not well suited for peaking capacity. The Binga project, however, just below Ambuklao, has a small reservoir useful only for daily or weekly load-factor regulation. Therefore there has been a tendency to consider these two projects as one, with peaking capacity largely at Binga as far as ultimate operation is concerned. On the basis of lowest net head (380 ft) a production of 51,000 kw is assumed. Three 25,000-kw generators are being installed, with 50,000 kw as the basic installation and the remaining 25,000 kw to take care of higher flow during the wet season and to serve as reserve capacity (Fig. 4).



Fig. 5. Maria Cristina Falls, in a Niagaralike gorge, was considerably diminished in volume by diversion through Linamon River to ease construction of power facilities. Eventually the entire river is to be diverted through penstocks bypassing the falls.

For some years to come Ambuklao will be the only operating plant. During this time there will be no need to draw down the reservoir for downstream plants, and Ambuklao production can be maintained at a minimum power capacity of 60,000 kw to meet the prime power demand of Manila and the Baguio mining district. When the complete project is in operation the total output with minimum reservoir levels is expected to be 324,000 kw, and total production with full reservoir levels should be 430,000 kw.

An earth- and rock-fill type of dam 432 ft high and 1406 ft long at the crest, with a gated spillway, is being constructed to create the Ambuklao reservoir. Because of the flood problem, an underground powerhouse was decided upon, with both powerhouse and tailrace tunnels located in the more uniformly solid diorite.

Beyond the mouth of the mountain gorge the Agno flows across the plains of Pangasinan, one of the major agricultural areas of Luzon, which, nevertheless, is not fully exploited because of lack of irrigation. Plans for irrigation of about 30,000 hectares (about 74,000 acres) in this area were interrupted by the war, for the Ambuklao project is not a prerequisite to a plan concerned primarily with wet-season irrigation to insure and increase the rice yield at that time of year. The Ambuklao project, however, makes possible a second or dry-season crop on about 20,000 hectares of irrigated land.

Floods in the Pangasinan plain area, which is quite heavily populated, cause an estimated annual

loss of 1,500,000 pesos (approximately \$750,000), with 8,500,000-peso losses incurred in the period 1935-37. Early wet-season floods will be absorbed by the low reservoirs of the project, and late wet-season floods should be moderated at least.

Maria Cristina project. By far the best power source in the Philippines is the Maria Cristina Falls site and the Agus River-Lake Lanao basin of which it is a part. A total production of 750,000 kw is expected from the ultimate basin project, with 200,000 kw coming from the Maria Cristina site alone. It is estimated that electricity can be produced and sold here for 0.3 ct/kw hr.

In contrast to the Ambuklao project in Luzon, with its great seasonal variation in water supply, difficult terrain, poor reservoir sites, and a ready nearby market, the Maria Cristina area has a large natural reservoir, steady water supply throughout the year, a favorable terrain, but no developed market. As a result the project also includes a government-sponsored fertilizer plant that will require the greater portion of the initial power production.

Located on the northwest coast of the main body of Mindanao, Maria Cristina is in a position to make possible the modern development of one of the most naturally productive sections of the Philippines. The area is essentially a recently uplifted lava plateau, with layers of dark basalt interbedded with deposits of conglomerate. A narrow coastal plain fringed with coral intervenes between the edge of the upland and the sea. Streams tumbling over the plateau edge have retreated by undercutting, creating narrow deep gorges with vertical falls. The most prominent of these is Maria Cristina

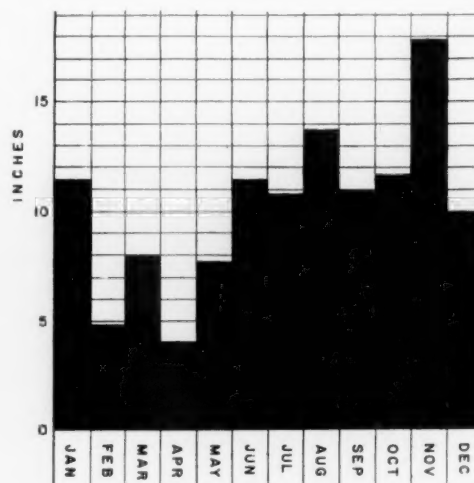


Fig. 6. Annual rainfall in the Lake Lanao area reflects the influence of location near the equator. Relatively even distribution is an asset to power generation and largely removes the flood and irrigation problem. [Source: Philippine Air Lines. Data from Philippine Weather Bureau]

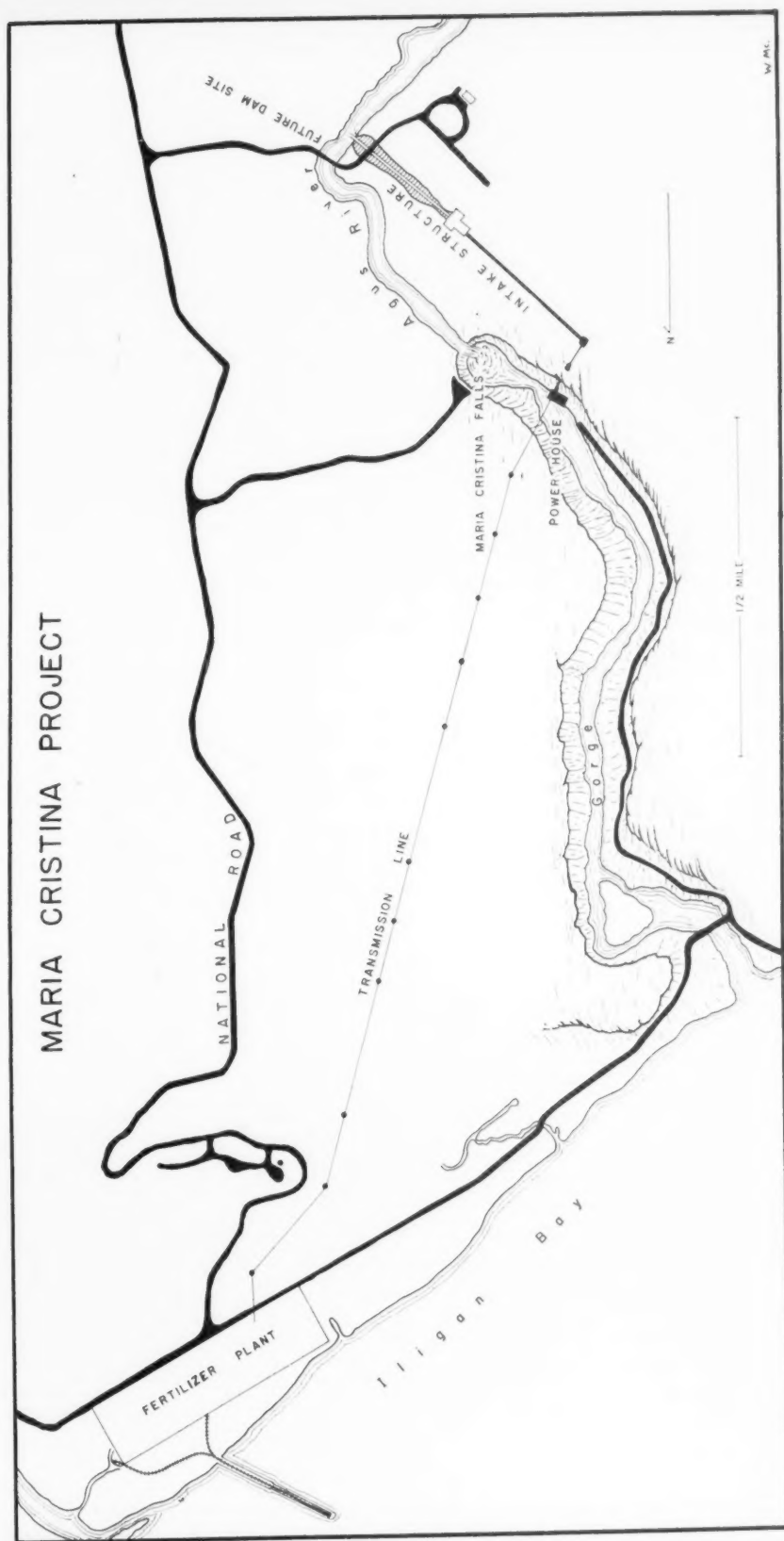


Fig. 7. Maria Cristina power project is a small-scale near-replica of the Schoellkopf power station at Niagara Falls, New York. Water is diverted around the falls by canal, falling through a penstock into the gorge below. Fertilizer plant will absorb most of the initial power production.



Fig. 8. Maria Cristina power station was constructed to contain three 25,000-kw generators, but only one has been installed. Planned additions to the station will produce 200,000 kw. [Courtesy National Power Corporation]

Falls, a short distance back from the sea on the Agus River (Fig. 5). Dropping 3 times the height of Niagara Falls, although probably with only one-third of the volume, the Agus River has created a scene of entrancing beauty. A thick growth of rain forest clothes the approaches to the gorge, while ferns and luxuriant vines and bushes, nourished by rows of springs issuing at the contact of basalt and conglomerate, cover the sheer walls. The river rushes in a wild torrent, filling the entire bottom of the gorge. Above the falls the plateau rises steadily toward the high interior of the island. The countryside is rolling, with volcanic prominences here and there in the distance. Lake Lanao, source of the Agus River, is a beautiful lake of volcanic origin well back on the interior plateau, forming a natural reservoir of large and dependable size. Rich soils from the young basalts and uniform rain conditions throughout the year are reflected in the bountiful production of fruits and crops beyond comparison in most of the Philippines. In fact, it is from Iligan, a tiny frontier port near Maria Cristina, that the bulk of the bananas for Manila markets is obtained. On the higher elevations toward the interior, the colorful Moro farmers raise corn instead of the usual rice, and herds of cattle on the green and rolling countryside remind one of the Appalachian Plateau of southern New York State. It is difficult to realize that the latitude is only about 8°N, despite the presence of palm and banana trees (Fig. 6).

Since the Maria Cristina Falls and resulting gorge resemble the falls and gorge of the Niagara River, it is perhaps not illogical that the power development should follow lines practically identical

to those revealed by American and Canadian power plants along the Niagara. Basically the technique is very simple. As at Niagara, the river makes a right-angle turn just above the falls. A canal diverts water from the upper river, crossing the angle to the gorge, thus bypassing the falls. From the canal water drops through a large penstock to a powerhouse in the gorge below. After motivating the turbines, the water escapes through the tailrace into the lower river (Fig. 7).

The actual installations reflect some of the non-engineering problems involved. The entire project is financed by Philippine capital alone, a rather difficult undertaking for the newly established government with its host of war-inherited problems. It is also a fact that there are no present markets awaiting the power development. In a sense this is a frontier area, at least for the Christian Filipino, for the Agus Valley is well populated with Moro farms and villages, and Dansalan, at the outlet of Lake Lanao, serves as an important Moro capital. The Moros have no interest in the type of industrial development or life required in the utilization of Maria Cristina power. Rather, they stand to lose their land and way of life as modern developments operated by and for the Christian Filipino move into the area. Moro raids have been frequent enough to require considerable watchfulness. At any rate there are no indigenous towns or industries in the area at present sufficient to utilize even a fraction of the power.

In view of this, a powerhouse large enough to

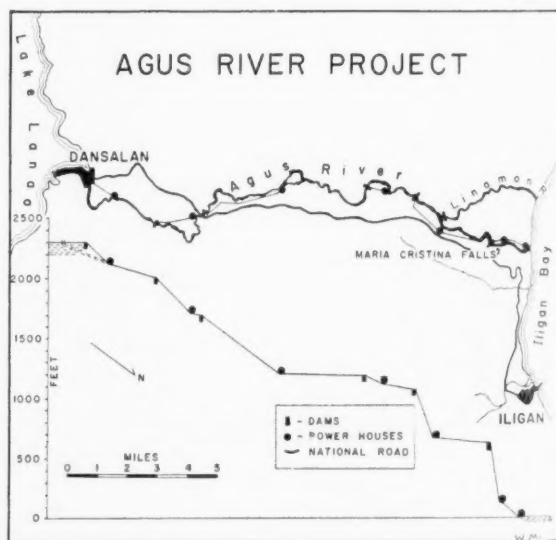


Fig. 9. The entire Agus River-Lanao Basin project will contain six dams and seven power projects producing 750,000 kw of electricity. A large-scale industrialization of the frontier area is visualized.

hold three 25,000-kw generators was constructed, but only one generator was installed. As markets develop it is planned to install two more generators and eventually to add to the powerhouse until a possible 200,000 kw can be generated (Fig. 8).

To insure a market for the power, as well as to provide much-needed fertilizer, a 50,000-metric-ton capacity ammonium sulfate fertilizer plant was constructed by the Philippine Government on the coast about 2 mi from the power site. In addition NASCO (National Shipyards and Steel Corporation) is constructing a steel-rolling mill nearby (4). Since the fertilizer plant alone requires 22,000 kw it seems that a power shortage, rather than a lack of market, will soon be the problem.

An interesting aspect of the Agus River course is the fact that the river bifurcates 4 mi or so above its mouth and empties into the sea through two separate channels. The smaller, known as the Linamon River, also has a falls similar to Maria Cristina Falls, though considerably smaller. To facilitate construction at the Maria Cristina site, a major portion of the Agus flow was diverted through the Linamon channel. Later, however, plans call for complete diversion of the Linamon through the Agus channel for utilization of the entire river at the better Maria Cristina site. A dam will also be built in the rapids just above the falls to further increase the head, and a total of seven power projects requiring six dams will eventually be installed (Fig. 9).

Conclusion. Philippine plans for hydroelectric development are sound, practical, and desirable. Accomplishment is difficult, however. Beyond the

obvious problems of short dollar supply, inflation, and internal and international disorders, there are specific problems of local character. Both the Maria Cristina and Ambuklao projects are in relatively isolated semifrontier situations. If a simple tool such as a reamer is broken, the engineer cannot dash over to the nearest hardware store, for there is none. He has to send to Manila or perhaps to the United States for a tool. There is no pool of skilled workers, such as bricklayers or welders. The few persons with such skills are likely to prefer employment in Manila rather than in the Moro country of Mindanao or the Igorot-inhabited Mountain Province of Luzon. In time this situation will improve, for the projects themselves are actually training programs. A tendency to ignore the need for replacement, repair, and alteration of original plans has handicapped the actual construction and possibly will handicap the future operation of the projects. Nevertheless, an essential step is being taken to bring about industrialization, better use of natural resources, national self-sufficiency, and improvement in standard of living for the Philippine Republic.

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Science is the great instrument of social change, all the greater because its object is not change but knowledge, and its silent appropriation of this dominant function, amid the din of political and religious strife, is the most vital of all the revolutions which have marked the development of modern civilization.—A. J. BALFOUR, Decadence (1908).

Why Science Attachés?

ROBERT L. LOFTNESS

Dr. Loftness, who is a project engineer in the Atomic Energy Research Department of North American Aviation, Inc., Downey, California, served as science attaché at the American Embassy in Stockholm, Sweden, during the period 1951-53. He was educated at the College of Puget Sound, the University of Washington, and the Swiss Federal Institute of Technology, and was a research chemist in the Radiation Laboratory of the University of California.

WHAT is a science attaché? What does he do? These two questions were asked innumerable times during my 2-year assignment at the American Embassy in Stockholm. They have been asked as often since my return to the United States.

The questions were asked in all sincerity. When asked by scientists they were toned with skepticism; what possible service could a Foreign Service Officer render to well-developed international scientific circles? When asked by laymen the questions were tinged with amazement; do foreign affairs and science have anything in common? As we discussed the questions, these people began to understand the reasons why science and foreign relations are interrelated. They began to see why many governments have appointed science advisers and science attachés. They realized that the pace of scientific progress is quickening, and that this progress is frequently creating problems in foreign affairs.

Whom Do Science Attachés Serve?

A science attaché has three basic functions; he is an adviser on scientific matters, a reporter of scientific events, and a representative of this government in scientific affairs. He serves not only his government but also the individual scientists of his country. The demands on his talents come from varied sources. The demands must be filled with an understanding of the background and interests of those making them.

Scientists all have a direct interest in scientific data and news. Hundreds of scientific and technical journals are published to provide this information. Publications, however, often lag 1 or 2 years behind the completion of experimental work. Much research, especially of an applied nature, is never published. Much is published in foreign languages and never gets the attention it deserves in English-speaking countries.

Scientists want help in making contacts and in financing visits and fellowships. They are interested

in the proceedings of scientific congresses. They benefit by help in overcoming legal barriers to the exchange of personnel and matériel.

It is true that there are some scientists who need very little liaison help. Widely traveled prominent scientists have all the contacts they can possibly maintain. They attend numerous international scientific meetings. Their every word is published. But only a small percentage of the world's scientists achieve such positions. Most scientists are not blessed with international fame, and they do not have unlimited funds for travel. Younger scientists and those who have come across promising ideas in fields bordering their own, need liaison help.

Our responsibility to scientists, both American and foreign, was to provide a point of contact, to provide information, and to act as a catalyst in promoting the exchange of personnel and information.

The relationship and duty of a science attaché to his government has a number of facets. There is a dual liaison and adviser function. There is an interest in political and economic information as well as scientific data. There is a wide audience of specialists and experts of all kinds.

The United States Government has many agencies and activities interested in foreign science and technology. Some activities are directly concerned with the exchange of scientific and technical information and personnel. These include the Fulbright and Smith-Mundt programs, the Point Four program (now under the Foreign Operations Administration), the Mutual Security Administration, and the National Science Foundation. Other agencies, engaged in extensive research activities of their own, are interested in reports on scientific research abroad. In this group are the Departments of Agriculture, Air Force, Army, Commerce, Interior, and Navy, as well as the Atomic Energy Commission, the Public Health Service, and the National Advisory Committee for Aeronautics. The Departments of Agriculture, Air Force, Army, Navy, and the Public Health Service find liaison so important

that they have their own scientific and technical liaison officers abroad.

The liaison function with regard to the government is filled partially by the same types of activities that serve individual scientists. There is an additional interest, however, in organizational and managerial problems. There is a requirement for reporting on some aspects of basic and applied research that are largely government domains—weather, geology, geography, agriculture, forestry, and others.

The position of a science attaché as an adviser on science in foreign affairs is a unique one. The Embassy in Stockholm was organized, as elsewhere, mainly around political and economic affairs. This was reflected in its reporting. Other aspects of foreign affairs were, to be sure, appreciated. They appeared to be subsumed, however, in the political and economic complexion of foreign relations. It was perhaps for this reason that my nonscientist colleagues appeared to be more sympathetic in their understanding of technical developments and their connection with foreign relations than in basic research problems.

The effect of science and technology upon foreign policy is a relatively long-range affair. There are not many pressing scientific crises demanding immediate solution. There have been in the past, however, a number of scientific and technical developments that have significantly affected foreign affairs.

The Haber process for the fixation of nitrogen from air was discovered and developed in the years 1908–14. Its importance to the German war effort was not given sufficient emphasis. The process provided explosives, however, to continue the war for 3 years longer than had been calculated on the basis of Germany's prewar supply of Chilean nitrates.

The principle of the submarine was known long before World War I, yet its vital war role was not widely accepted until intensive submarine warfare was a fact.

Our foreign policy toward rubber-producing areas has markedly changed since the development of synthetic rubber. The invention of rayon, nylon, and other synthetic fibers has caused major economic disturbances in silk-producing areas. The invention of the Haber process for fixing nitrogen not only lengthened the war in Europe but also had severe economic repercussions in Chile, because it destroyed the market for one of her chief exports. The market for American domestic wood alcohol dropped suddenly in 1920 when a German ship docked in New York with a load of synthetic

wood alcohol which sold for half the usual price.

The first jet aircraft and the V-1 and V-2 rockets burst upon the military scene with seeming surprise—yet the principles and even the state of development in Germany were known before the last war.

The most significant single scientific development to affect foreign affairs was, of course, the discovery of nuclear fission. It has resulted in a weapon of unsurpassed power and has kept postwar diplomacy under tensions unprecedented in history. The lessons learned from World War II scientific developments keep military organizations at sharp attention for new developments. New discoveries of military importance will probably receive careful evaluation.

There remain, however, many nonmilitary scientific developments that might alter our economic or political relationships with other countries. These include atomic and solar power; simple, effective birth-control methods; the ability to conquer certain plague diseases; new techniques for producing food; new educational techniques; and others.

Science attachés should collaborate with economic and political officers in evaluating the meaning of scientific and technical developments in terms of long-range economics and politics. This is a large, difficult, and precarious task, somewhat afiel from ordinary scientific pursuits, but one of paramount importance.

Duties as Originally Defined

The Science Office within the State Department was established after an extensive survey had been made of the needs of both government and private agencies. This survey endeavored to answer two questions: "first, how can the potentialities of scientific progress be integrated into the formulation of foreign policy, and the administration of foreign relations, so that the maximum advantage of scientific progress and development can be acquired by all peoples? And second, how can foreign relations be conducted in such a manner as to create the atmosphere that is essential to effective progress of science and technology?"

This survey resulted in a report entitled *Science and Foreign Relations*. [Department of State Publication 3860, Lloyd V. Berkner, Ed. (May 1950)]. This report constituted the major instruction manual for the first science attachés. It contained a wealth of information. Many of the thoughts in this article have been drawn from this report. The difficulties we encountered as science attachés were largely anticipated by this report.

As stated in *Science and Foreign Relations*, the functions of the science attachés were to include:

Reporting on significant scientific and technological developments

Assistance in the exchange of scientific information

Assistance in the exchange of scientific persons

Assistance in the procurement of scientific apparatus, chemicals, and biologicals

Cooperation with all United States groups abroad having programs with scientific and technological aspects

General representation of United States science

Scientific and technical advice to the Embassy staff

Arrangement for collaborative research projects between United States and foreign scientists

General promotion of better understanding between United States and foreign science.

Duties Actually Performed

The duties, as outlined, were well chosen. In practice we had occasion to perform all of them. The particular emphasis we placed on these duties arose from the nature of scientific activities in Scandinavia and upon the fact that we were the first science attachés in this program to be assigned to this area.

The bulk of the first 2 years' work was devoted to a basic survey of scientific activities in Scandinavia. Most of these data, submitted for the first time during those 2 years, need not be comprehensively reviewed again for perhaps 5 years.

We provided information about American scientific activities to Scandinavian scientists upon request.

We assisted in the exchange of scientific personnel through existing government channels and private foundations.

We attended and reported on scientific congresses.

We helped arrange for American scientific films to be loaned to universities in Scandinavia.

We expedited export permits for critical scientific equipment from the United States.

We spent some of our time in consultation with other members of the Embassy staff on their problems.

There is, at any Embassy, a constant flow of problems that merit the attention of a science attaché. In Stockholm one such problem concerned a fireproofing agent. An inventor came to the Embassy with a claim of having discovered a new and outstanding fireproofing agent. His hope was to receive help in the commercial exploitation of this invention in the United States. A demonstration

was made. A blowtorch flame was applied to various pieces of building board that had been treated with the agent. By observation, the fireproofing agent was indeed better than nothing at all. But closer inquiry about the nature of the agent revealed that it was a mixture of borax and boric acid. These have both been common fireproofing agents since the last century! The inventor had not bothered to investigate the previous art of fireproofing agents. He was asked to have scientific comparison tests made at the local government testing station to provide sound technical data in support of his claims. He wasn't heard from again.

As the science attaché program becomes more firmly established, the responsibilities of the science attaché will become more clearly defined. Cooperative consultations with other Embassy staff members on problems involving science and foreign relations will broaden in scope. General reporting work will probably diminish, while scientific requests for information will increase.

Where Should Additional Emphasis Be Placed?

Based upon experience in Scandinavia and without intimate knowledge of the problems at the Washington end of the program, I would make the following suggestions:

Articles should be published in established journals. Each attaché should write two or three articles each year for regular scientific journals. The articles could be about specific research programs, scientific congresses, problems of science and foreign affairs, or other appropriate subjects. The articles should appear in widely distributed journals rather than as government dispatches. Perhaps an officer in the Washington Science Office should be delegated to coordinate the publication of such articles; determine journal requirements, edit, and arrange for appropriate clearances.

English-language handbooks on scientific research should be published. Where summaries of research in progress are not available in the English language, the science attaché should encourage publication by local groups of handbooks or brochures describing scientific research in various fields. Such handbooks should include basic information about laboratories, universities, government scientific organizations, academies, and so on, and should be revised every 5 years or so. If necessary, the science attachés should take an active part in writing such handbooks.

Files of information on American research should be maintained at posts abroad. The science attachés should be supplied with current information about research at universities, academies, research institutes, and industrial and government labora-

tories in the United States. This information is invaluable to the attaché in answering requests for information about American research. The science offices should be on the mailing lists for such publications.

The economic and political aspects of scientific progress should be emphasized. Although this task is fraught with difficulties, it should be attempted. It is the job that is most unfamiliar to scientists, yet it is the job of most significance to the State Department. Efforts should be made to evaluate expected progress in individual sciences as they might change the economics or politics of each country. This means consideration not only of progress within the country itself but also of progress in other countries where political or economic ties exist.

A career for some science attachés should be established. As in any permanent service, continuity is important. It is difficult to find prominent scientists who are able to take a year or two from their scientific specialty to be science attachés. They cannot leave their research work unsupervised for long periods. Often their university or organization is reluctant to permit extended absences.

It is the prominent scientist who has the most to offer foreign scientists and who will most certainly command their respect and gain their confidence. These are the men who would add the most to our scientific prestige abroad. Yet hardly do they get started on the job then they must leave. Continuity is lacking as well as the know-how, know-whom, know-where, and know-what that come with experience on any job.

There is a need around these temporary science attachés for a core of career liaison officers to provide the continuity necessary. A place should be established within the framework of the Foreign Service for perhaps six career science attachés. These should be men of broad scientific training who have an interest in foreign affairs. Although specialists, they should have opportunities for advancement equal to other regular foreign service officers. There is no reason why these men, after acquiring experience, should not fit into the regular

corps of officers. Their scientific background would result, naturally, in assignments largely of a special nature—those of science attachés, delegates to scientific meetings, representatives to organizations such as UNESCO, and so forth.

Prominent scientists should be appointed for special short term jobs. These men can usually afford to spend only limited periods away from research activities. They should be given, therefore, special assignments of 3 months to 1 year. They should be allowed to concentrate upon the most important aspects of the job. The routine work should be carried by the career attachés.

The knowledge of visiting American scientists should be utilized. American scientists, traveling abroad at government expense, should be asked to write a trip report. These reports should contain information about research, problems of the foreign scientists, fellowships available or desired, and so forth. The report should be supplied to the science attachés as well as to the government agency sponsoring the trip.

Activities should be extended to other countries of the world. At the present time, science attachés are stationed only at London, Bonn, Paris, and Stockholm. These European countries have first-rate scientific organizations, and there are wide areas of contact. However, the development of science is important in all countries, and we should take an active interest. The report *Science and Foreign Relations* recommended that attachés be sent to all areas of the world. This should be done. The duties of science attachés in "underdeveloped" countries would be different but extremely worth while.

Both basic and applied sciences should be emphasized. The initial emphasis has been largely upon the basic sciences. Although important discoveries occur constantly in basic science, it is the application of science that affects politics and economics. Attention should also be given to applied research. It is at the applied research stage where results of immediate economic and political importance are found.



It requires a very unusual mind to undertake the analysis of the obvious.—A. N. WHITEHEAD.

SCIENCE ON THE MARCH

ATOMS FOR EXPORT

ENGLAND continues as a world leader in producing radioactive material for peaceful international use. Because of the growing demand from home and abroad for these valuable materials, so important in medicine, science, and industry, a number of new buildings, especially designed to accommodate isotope production processes, have been added to the British Radiochemical Centre at Amersham, near London.

Until now, radioactive isotopes were almost entirely produced with simple laboratory apparatus that required some direct manipulation and which was housed in general-purpose buildings. Today, the demand for the more important isotopes is so great that it has become necessary to construct individual chemical plants, completely screened and remotely operated, and to install them in buildings designed for the purpose. Improved facilities for dispensing, packing, and dispatching the products are also included.

These pictures, taken in the laboratories at Amersham, show some of the steps in the preparation of radioactive materials. Waste solutions of

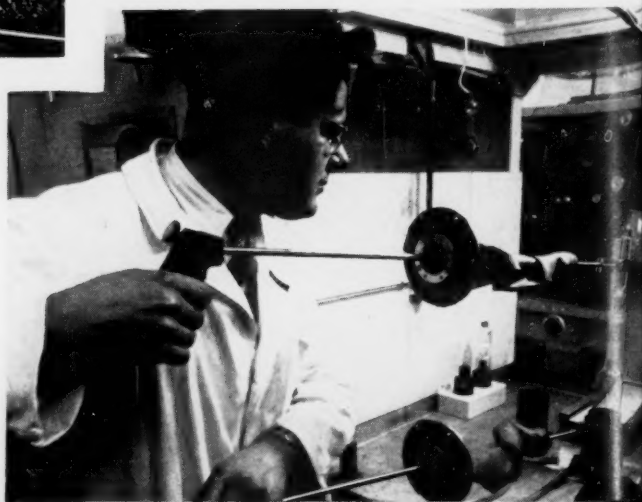
fission products from which the plutonium and uranium have been removed are received at Amersham from the British atomic production factories in northern England. These are the raw materials from which several important isotopes are separated.

The most versatile radioactive isotope, carbon-14, made by irradiating nitrogen with neutrons, is used in medical and biological research. The target material (aluminum nitride) is prepared at Amersham, sealed in aluminum containers, and sent to the production piles at Windscale to be irradiated for about a year. The active material is then returned to Amersham where the carbon-14 is extracted from it. Another material being produced is strontium-90, now widely used industrially in such instruments as thickness gages; it is also used for the treatment of skin diseases.

During the last 5 years, 12,500 radium needles have been manufactured at Amersham for the treatment of cancer, and of these only one needle has been reported faulty. [Photographs by courtesy of the British Information Services]



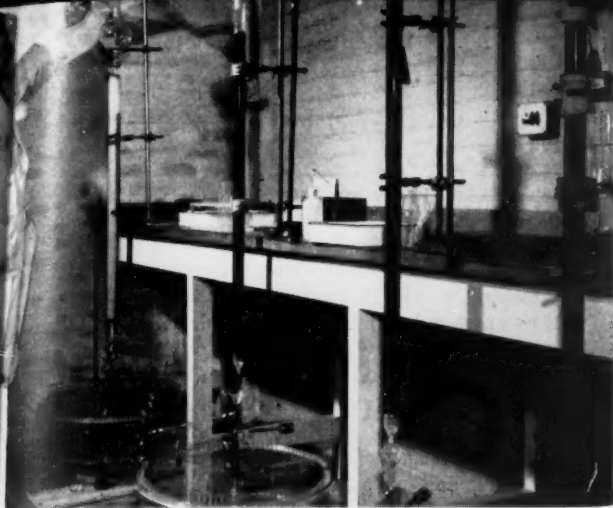
A chemist, protected by a screen of transparent plastic 1 in. thick, handles radioactive phosphorus in one of the new production plants.



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The separation of individual amino acids from radioactive protein is done by column chromatography. To separate the 17 amino acids contained in the protein, about 2000 fractions have to be collected and measured. Here the chromatographic columns and the automatic fraction collectors are shown in use.

The production of radiation sources for use in industrial instruments such as thickness gages is an important part of the work. Here an operator, protected from the beta radiation by transparent plastic screening, rolls a metal foil containing strontium-90.



One of the new laboratories is used for the separation of radioactive isotopes from fission products, and strontium-90 and cesium-137 are produced. The two cells shown in the photograph at the left contain equipment that is screened by 2 ft of concrete and is operated entirely by remote control.

An operator measures the strength of a beta-ray appliance containing strontium-90. It will be used in Australia for the treatment of skin diseases.



BOOK REVIEWS

Advances in Genetics, vol. VI. M. Demerec, Ed. Academic Press, New York, 1954. ix + 488 pp. Illus. \$9.80.

VOLUME six in this series comprises eight reviews that are, on the whole, rather specialized. Contributions in the bacterial, viral, and biochemical fields are notably absent, and the one paper on *Neurospora* treats the relatively neglected area of map construction. This onerous task is undertaken by Barratt, Newmeyer, Perkins, and Garnjobst, who have with laudable altruism stayed off the bandwagon of biosynthesis. The book contains an excellent discussion of mapping functions, maps of the linkage groups, tabular data from all available sources, and lists of mutants. For those in the *Neurospora* field this will be, for some time, the indispensable standard reference.

The paper of most fundamental interest is undoubtedly P. Michaelis' 106-page summary of cytoplasmic inheritance in *Epilobium*, the first really extensive account of this scholarly and persistent program to appear in English translation. It is rather hard going, however, and one is left with the feeling that conclusions at the level of theory are still premature after more than three decades of research. There is no questioning the genetic continuity of some cytoplasmic constituents, but despite the promising line of investigation offered by plasmon alterations in this plant the problems of analyzing the plasmon into specific components and determining its material bases have remained insoluble. The broad generalizations that are proposed under the warning label of "the new hypothesis" seem, to me at least, to be without adequate foundation.

Bentley Glass, in discussing genetic changes in human populations, skates the thin ice of speculation skillfully, despite great difficulties peculiar to this subject. One might expect gene frequencies in man to have been so erratically affected by historical accident as to be scarcely comprehensible in terms of underlying principles, yet several existing situations have been reasonably explained by simple models of gene flow in space or time. Assumptions often made in determining human mutation and allele frequencies are challenged, and the role of selection and genetic drift is discussed; these are matters on which our understanding has been considerably augmented by Glass' own demonstrations of the compensation phenomenon and of genetic drift as revealed by age-group analysis.

A more specialized aspect of human genetics, that of the newer blood groups, is reviewed by Philip Levine, allowing the nonspecialist an opportunity to catch up momentarily with the rapid progress of discovery in this field. New factors are described, both related and unrelated to previously known groups, and in many cases important in maternal-fetal incompatibility. Particularly interesting from the genetic viewpoint is the puzzling mode of inheritance of the

Lewis factors, their relationship to the secretor character and chemical similarity to the A and B substances. The unsettled nomenclature of the Rh series emphasizes the need for criterions to distinguish between pseudo-alleles and multiple-alleles, and perhaps the practicability of searching pedigrees for rare instances of original recombination.

Probably the world record for the greatest number of alleles at a locus is held among incompatibility genes in certain plants. The modes of inheritance, mutability, and physiology of systems leading to obligate outbreeding are discussed by D. Lewis in a review of comparative incompatibility in angiosperms and fungi. Phenogenetic studies in this area, such as Lewis' serologic analysis of pollen, provide a promising approach to fundamental problems of gene action. Charles L. Remington's article on the genetics of *Colias* will be of special interest to the systematist, since it deals with the genetic bases of polymorphism in natural populations of *Colias* and other butterflies. Finally, two papers are mainly concerned with practical applications. One is by John Hancock, on monozygotic twins in cattle and the useful comparison of within-set and between-set variance in judging the heritability of economic traits; the other is by Alan Robertson, on artificial insemination and livestock improvement, in which it is concluded that the facilitation of progeny-testing programs should become the most valuable application of this powerful technique.

K. C. ATWOOD

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Tomorrow Is Already Here. Robert Jungk. Simon & Schuster, New York, 1954. x + 241 pp. \$3.50.

THE author is a Swiss journalist who has led a varied life. As a Jewish student in Berlin, he was arrested the day after the Reichstag fire but was freed very soon. He left Germany and traveled in many countries, studied at the Sorbonne, and completed his formal education at Zurich. Between 1947 and 1953 he traveled throughout the U.S. as a correspondent for some Swiss newspapers. *Tomorrow Is Already Here* represents the results of these journalistic activities. It is being published simultaneously in 11 countries. The picture of America and Americans which it paints thus will be widely disseminated and will add its bit to the present complex of notions about the U.S. and its inhabitants.

The author makes no attempt to describe the nation as a whole. Instead he presents some 25 highly subjective and impressionistic vignettes arranged very loosely in six groups, each group being concerned—more or less—with some very modern aspect of American life. Scientific discoveries, gadgets, research ambitions, and their impact on people and ideas are the themes of the short essays, which are written very

simply in the first person. Jungk pictures the United States as he saw them, and he seemed to have looked at them through science-fiction spectacles. He was frequently startled, generally a bit breathless, but always clear and fair. The book definitely gives the impression that tomorrow is already here.

The nearest approach to an over-all philosophic theme is given in the first chapter. It is that Americans, without knowing it, are being engulfed in a totalitarian revolution and their basic liberties are being threatened. The threat comes, not from any paranoic dictator or from the plotting of any subversive group but from the basic changes taking place in science and, consequently, in society. The new American frontier is now the universe itself, which must be conquered and reshaped if necessary to advance the American standards of living. The task requires great organization, of course, and organization always decreases personal liberty. The Americans may even have gone so far as to be guilty of *hubris* and *hubris* is always punished. Perhaps God does not want His universe improved!

CONWAY ZIRKLE

Botanical Laboratory, University of Pennsylvania

Minerals for Atomic Energy. A guide to exploration for uranium, thorium, and beryllium. Robert D. Nüniger. Van Nostrand, New York-London, 1954. xii + 367 pp. Illus. + plates. \$7.50.

THIS book fulfills admirably the purpose for which it is written: namely, "an authoritative guide for layman or expert to successful prospecting for atomic minerals." The text is divided into three principal sections (I, what to look for; II, where to look; and III, how to look), together with a series of appendixes (pp. 223-347) packed with useful information in somewhat smaller type. The only thing we are not told is just where to stake a claim in order to get rich quickly. This, however, would spoil the fun. All that is necessary to get started is to pack the car or truck (pp. 165-168), not forgetting that *second* spare tire and the treatment for rattlesnake bite, purchase the Geiger counter or scintillation counter (pp. 176-180), prices range from \$30 to \$1000 or more (pp. 188-189), (in some areas they may be obtained in drugstores but in case the operator does not wish to rely wholly on this source of supply, the addresses of some 40 manufacturers and distributors are listed, pp. 268-269), and obtain good maps (p. 167).

To write a book for both the layman and the expert is usually not easy to do satisfactorily. If we picture the layman as a husky individual with no more than a very general scientific knowledge, he may well find the present book a rather tough proposition, but by judicious skipping of certain parts he will find clearly set out the essential information on geologic formations, where uranium, thorium, and beryllium are generally found, how to recognize them, and how to equip himself for the job. The expert is not likely to be expert on all aspects of this new activity. He may be a good

geologist and/or mineralogist but know little about counters and nothing at all about regulations governing prospecting. Or, he may have years of experience as a prospector for other minerals and know nothing of the particular aspect of looking for uranium. All such experts will find a good deal of information that can scarcely fail to be of interest to them.

Perhaps more important than even the information itself is the clear guidance that the book provides to sources of additional information and to professional assistance. The author has exercised a wise restraint in the amount of detailed information given on the uranium and thorium minerals. This knowledge is still in the process of elucidation and is often very complex. The tables of radioactive minerals (pp. 239-259) are grouped according to the colors of minerals, and, when the color is variable, a mineral is listed more than once. The information provided is the kind likely to be useful in field identification, and more detailed items, such as optical properties, x-ray data, and chemical compositions, are largely (and quite properly) omitted.

The book is well printed and illustrated and is embellished with several colored plates. The style in which it is written makes it pleasant to read. The separation of the general text from the factual material given in the appendixes has enabled the author to cater usefully to both classes of readers, and, at the price it is offered to the public, it is an excellent "buy."

G. W. BRINDLEY

Mineral Sciences Building,
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America's Resources of Specialized Talent. A current appraisal and a look ahead. Report of the Commission on Human Resources and Advanced Training; Dael Woffle, director. Harper, New York, 1954. xviii + 332 pp. Illus. \$4.

THIS commission was appointed by the Conference Board of Associated Research Councils in 1949 to make a comprehensive survey of the specialized manpower resources and requirements of the United States. We are now and in the future greatly dependent upon those citizens "who are educated, intelligent, able to work with ideas, and qualified to plan and understand and direct the nation's complex web of industrial, technological, social, scientific, and governmental institutions and problems."

The commission's report unquestionably is the most comprehensive treatment of the problems of the development and use of human talents ever published; it provides the most complete data thus far assembled in a field where much necessary information is still lacking. Furthermore, its interpretations are broadly conceived and recognize the variety of conflicting forces and interests involved.

The demand for specialists has grown much more rapidly than our population. The report estimates a growth from 13,000 specialists per million population in 1900 to 23,000 per million in 1950. The social and

economic trends responsible for this growth in past decades are expected to continue, thus creating an even greater demand for specialists.

How can this need be met? The commission finds possible solutions in two directions, through encouragement of a larger portion of able youth to enter and to graduate from college and through better utilization of many of those who are trained. Of the top 16 percent of high-school graduates only about one-half enter college. Better educational guidance in schools, more adequate scholarship support, and parental and community encouragement would probably double the proportion of competent high-school graduates who enter college.

Women, men over 65, Negroes and other minority group members who have received specialized training are frequently unemployed or engaged in work that does not fully utilize their trained talents. Policies are suggested to improve their utilization and thus help to relieve the shortage of trained manpower. Furthermore, a larger number of assistants and technicians would increase the effectiveness of engineers and scientists and free many of them from positions that do not call fully upon their training.

Replete with relevant data, carefully reasoned and well written, this report should be very influential in developing needed manpower policies.

RALPH W. TYLER

Center for Advanced Study in the Behavioral Sciences

The Causes and Treatment of Backwardness. Cyril Burt. Philosophical Library, New York, 1953. 128 pp. \$3.75.

THIS is an excellent little book, truly a measure of the excellence of the author whose professional life has contributed so much to the welfare and treatment of backward and retarded children. To a great extent the book is a summary of the knowledge in this field and it seems entirely fitting that it is addressed primarily to school teachers and educationists who are concerned with mentally handicapped children. It is, also, a measure of the true scientist in that it discusses many complex problems in the language of the intelligent layman. Often, one page of this book reduces a problem, formerly confusing and vague, to understandable terms.

There are eight chapters arranged methodically to give the problem for discussion, its historical and current setting, the methods of investigation, the role and importance of environmental factors, and the physical, intellectual, and emotional characteristics of retarded children.

While medical specialists might think that Burt is biased toward psychology in respect to the contributions of that science and profession, as compared with medicine, I detect no polemics and believe the treatment of problems that concern more than one profession to be entirely objective, disinterested, and well documented. In fact, in the discussion of emotional problems and psychotherapy there is merely the statement that treat-

ment following psychological principles of learning and training has been more successful than that which has been modeled after psychiatric practices.

It is good that Burt sets the record straight on such matters as the above, and that he reminds us that intelligence tests began with Galton rather than with Binet and that the first child guidance clinic in England considerably antedated Witmer's Clinic at the University of Pennsylvania.

While no destructive criticism seems possible, I believe that the reader will get the impression of somewhat more constancy of the I.Q. than may be the real case and it might have been possible and more helpful to have given more attention to etiology and the ways in which knowledge of etiology may guide training and treatment. The book is interesting reading as well as efficiently informative.

KARL F. HEISER

The Training School, Vineland, New Jersey

The First Australians. Ronald M. Berndt and Catherine H. Berndt. Philosophical Library, New York, 1954. 144 pp. Illus. + plates. \$4.75.

THE husband-and-wife team Ronald and Catherine Berndt are Australian social anthropologists who have contributed much to our understanding of the cultures of the Australian aborigines. They have engaged in extensive fieldwork in those regions of Australia, such as Northeast Arnhem Land, where the native ways of life still survive in approximately their pristine form and they have several substantial monographs to their credit. The present book is offered as their attempt at a generalized popularization of knowledge about Australian native culture and is not too successful. Like many specialists in many fields of human knowledge these authors are completely familiar with the detail of their material, but are unable to paint the over-all general picture in strokes sufficiently strong and decisive to convey to the casual reader what they want to convey. This book gives a picture—although somewhat confused—of what the natives around Oenpelli do as contrasted with what is done further east in Arnhem Land or on the Daly, but there is little firm generalization about what Australian aborigines do, still less about why they do it.

Depicting non-Western cultures and their "ethos" to the general reader requires an artistic, rather than a scientific, set of techniques, as the popular writings of Ruth Benedict and Margaret Mead have shown. The Berndts are unable to produce those broad, slashing, vivid approximations to the facts that make the work of Mead and Benedict so illuminating to the lay audience. This is the more noticeable because the Berndts know where the emphases in describing the aboriginal Australian way of life should be. The complete integration of every aspect of the culture; the complete fusion of past, present, and future; of natural and supernatural; of animals, men, and nature; the continuous cultural feedbacks whereby every action and every value reinforce and give more meaning to every

other action and every other value—these are the features that make Australian cultures, before their destruction by white contact, the classic examples of complete functional unity between the various phases of human living.

An Australian culture is almost algebraic in its symmetrical integration, with no loose ends and no discontinuities. The authors are well aware that it is this symmetry and completeness that constitute the crux of the Australian way of life but they fail to solve the descriptive problem of how it can be communicated to the Western reader, even by writers who understand it as thoroughly as they unquestionably do. Social anthropology has reached the point of understanding its material remarkably well, even of knowing what it wants to say about it, but still desperately lacks tools that will communicate its understanding of other cultures to the nonanthropological audience. *The First Australians* is an excellent example of the understanding, of the wish to communicate, and of the lack of communication between anthropologist and reader. The ordinary American, even if literate, will respond to this book by having his prejudice about naked primitive savages reinforced and will thank God that he is 100 percent American. It is not the fault of the Berndts; the task they impose on themselves is analagous to asking a tone deaf audience to understand and appreciate Beethoven.

There are numerous line drawings of aboriginal art and a number of good photographs, many of the latter rather spoiled by an apparent prohibition on any male nakedness. The calico loin cloth is *not* part of native culture.

C. W. M. HART

Department of Anthropology, University of Wisconsin

Geographical Essays. William Morris Davis. Douglas Wilson Johnson, Ed. Dover, New York, 1954. vi + 777 pp. Illus. \$5.50.

AN unabridged republication of a group (26) of William Morris Davis' essays that were selected and edited by Douglas W. Johnson in 1909, *Geographical Essays* includes many that are classic in geographic literature. Some of the papers date from 1889, yet they read with delightful freshness.

Approximately one-half of the essays and two-thirds of the book are concerned with problems of landform genesis, including Davis' theories of peneplanation, of valley development, and of the geographic cycle. Although some of the ideas he propounded have been modified and some refuted, these essays contain much that is of continuing interest. They reveal a keen imagination and skill in exposition.

One-third of the book contains essays on teaching methods and on the importance of geographic instruction in schools and colleges. Geographers are indebted to Davis for his notable contributions in the field. Many of his admonitions still apply: avoid the *omnium-gatherum*, "mere facts of occurrence and location have about the same rank in geography that words have in literature," cultivate the habit of looking for the mean-

ing of things, "grasp and teach the spirit, not the letter," stress those things that a cultivated intelligence would value.

One prediction made by Davis has happily proved inaccurate: "It is not to be expected that advanced geographical courses should be largely attended. They will never pay for the cost of their establishment." More than a little credit is due Davis for the fact that there are now dozens of geography departments not only paying their way but, more important, making substantial scholarly contributions in the academic world.

WILLIAM A. HANCE

*Department of Economic Geography,
Columbia University*

The Giant Cactus Forest and Its World. A brief biology of the giant cactus forest of our American Southwest. Paul Griswold Howes. Duell, Sloan, and Pearce, New York; Little, Brown, Boston, 1954. xxv + 258 pp. Illus. + plates. \$7.50.

ACCORDING to the author

The present book is especially for those thousands of annual visitors and residents who tour and play and hunt and camp on the mountains, in the valleys, and on the ranches, within the range of the giant saguaros.

The land area described is approximately south-central Arizona. Howes addresses himself to that vaguely defined person known as the layman. As a layman who has visited the saguaro desert once and several other of the American deserts occasionally, I recommend this book to the traveler who feels that the desert is a harsh and barren place, to be crossed as fast as his car will take him.

The author

. . . would tell us what to expect and what the possibilities of study in this realm were; . . . help us identify and learn; . . . illustrate most of the large and obvious things, as well as much of the minutiae. . . .

The book fulfills these purposes well. The plant and animal life of the area, from the largest to the smallest forms, is considered as a whole, and the interrelationships and interdependencies, where known, are carefully described.

I was surprised and shocked to read that the giant saguaro cactus is threatened by a deadly disease. It would be indeed a shame if it were to go the way of the magnificent chestnut of our eastern woodlands. In the chapter on cacti, a small table listing the differences between species would have been useful. The distinction between yuccas and agaves is well given. Although the impression is given that desert plants flower only in the spring, the summer visitor will find some plants (for example, prickly poppy, barrel cactus) in flower. Howes is to be commended for his reasonable and nonanthropocentric attitude toward the "blood-thirsty" eating habits of carnivorous animals. Among

the other interesting features are the emphasis on desert food cycles and the chapter on desert arthropods. However, the philosophical remarks on pages 186 and 217 might well have been omitted.

There are an excellent index and a list of references for more detailed information on all aspects of desert life. To the latter might be added the inexpensive U.S. Department of Agriculture publication, *Southwestern Trees*, Agriculture Handbook No. 9.

MORTIMER L. BLOOM

Brooklyn College

General College Chemistry. Andrew J. Scarlett and José Gómez-Ibáñez. Holt, New York, 1954. x + 645 pp. Illus. \$6.

THIS textbook follows the four editions of *General College Chemistry* by the late Leon B. Richardson and Andrew Scarlett. It represents, however, a very thorough departure both in content and in the order of presentation from its predecessors. The first half is devoted almost exclusively to the development of fundamental theory, followed in the latter half by the descriptive chemistry of the elements and their compounds. As the authors point out, "the arrangement of chapters in this text gives a systematic presentation, and at the same time permits great latitude in selection."

Such topics selected at random as the van der Waals equation, the distillation of liquid mixtures, *s*, *p*, *d*, *f* orbitals, ionization potentials, atom radii, and ionization of hydroxides in terms of ion radii will give some idea of the scope and thoroughness of this modern textbook. Yet it should be possible to omit many of these more difficult topics without breaking the continuity of the treatment. The subdivision of the descriptive chemistry into that of the elements and that of typical classes of compounds is another highly commendable innovation. In short this is a flexible, teachable, and thoroughly modern general chemistry textbook.

JOHN A. TIMM

Department of Chemistry, Simmons College

The Anatomy of the Migratory Locust. F. O. Albrecht. Athlone, London; de Graff, New York, 1953. xvi + 118 pp. Illus. \$6.

THE *Anatomy of the Migratory Locust* is well designed as a laboratory textbook in entomology. It also serves as a valuable source for original anatomical information.

From the world-wide point of view, the migratory locust is a happy choice for courses in entomology. It is available to more people than any other locust. Occurring regularly throughout much of Africa and Western Asia, it periodically forms enormous migratory swarms that may reach as far as Western Europe. These sometimes cause famine in their wake. It is larger than most American locusts and could be preserved easily in quantity for laboratory studies anywhere. I once purchased a large burlap bag of these

locusts, alive, for one dinar (\$2.80) in the bazaar in a Middle-Eastern desert village where they were sold for food. The British have been successfully rearing them for physiological and other studies.

The textbook is clearly illustrated with excellent black and white drawings and is well supplied with center headings on all topics. This may be somewhat extravagant in the use of paper but it makes for quick and easy reference. The drawings generally carry labels which suggest the word, a minor point but it makes for ease in use.

One might lift his eyebrows at the statement in the foreword, that "improved sanitation appears to have reduced the cockroach population" and that the cockroach therefore has become "difficult to obtain," as an excuse for using the locust over the roach, but either is sufficiently generalized in form to be of primary utility. A useful reference list of 34 titles and a practical appendix with concise laboratory directions concludes the work.

NEAL A. WEBER

Department of Biology, Swarthmore College

Ancient History of Western Asia, India and Crete. Bedřich Hrozný. Trans. by Jindřich Procházka. Philosophical Library, New York, 1953. xiv + 260 pp. Illus. + plates + maps. \$12.

THIS is the result of many years of research on the part of Bedřich Hrozný, of Charles University, Prague. His purpose, indicated in the preface (p. vii) is to tell the story of ancient Western Asia from the

... origins of the oldest known human culture leading on to the subsequent development of civilized life in Europe as well as other parts of the world. ... We can hardly imagine modern mankind without a knowledge of writing and Christianity, both of which originated in the Orient, that is, in Western Asia. But many other links connect our modern civilization with the ancient Oriental cultures, viz. the European arts, science (astronomy), and law which developed from the Roman Law; and even in technical and practical life we meet many elements which are of Oriental origin. The study of ancient Western Asia takes us to the very roots of our modern civilization; it investigates the origin of its individual components and traces their gradual development.

To carry out this task, Hrozný scrutinized the archaeological remains and other pertinent materials of a wide area. He begins by describing the life of early Near East, then the prehistoric periods in Palestine, Mesopotamia, Babylonia, and Egypt. The next several chapters deal with the Hamite-Semites and the Caspian peoples, the origin and the original home of the Samaritans and the earliest migration, as well as the earliest Sumero-Akkadian dynasties up to about 2400 B.C. In the next chapters, the heart of the book, the author attempts two things. On the one hand, he describes in greater detail and with a good deal of originality the contributions of the Sumero-Akkadian culture to the whole of the ancient cultural world, from

early Egypt through India to China. On the other hand, he attempts to show that from the middle of the 3rd century B.C. the first group of Indo-Europeans, among whom were the Hittites, originating from the Caspian region, also helped greatly in building the ancient old-world civilization. Outstanding characteristics of the Sumero-Akkadians were their city-culture based on a well-developed plow agriculture, metallurgy, the picture (and later the cuneiform) writing, and their astronomy and astrology. The outstanding characteristic cultural development of the early Indo-Europeans would seem to be in some way their acquaintance with metals and some manner of working them.

This is a very scholarly work in the tradition of the Orientalists. Its weakness lies in the author's inability or lack of interest in the functional relationship between elements of culture and in the psychological characteristics of peoples underlying their movements and conducts. For example, one branch of the Hamites was supposed to have become the ancient Iberians in the Spanish Peninsula because the language of the latter contains some "striking Hamitic elements," also because the Spanish bullfights "seem to be but the last reflex of the fight of the Sumerian heroes Gilgames and Enkidu with a bull," a fight that is "engraved in many different forms on Sumero-Babylonian seals," which are said to be "artistic representation of the process of domestication of the bull by man" (p. 49). The first portion of this observation is no sounder than the now thoroughly discredited logic of the diffusionist anthropologist W. J. Perry—that any similarity in form between two cultural elements found separately in diverse places is "proof" of their identity in origin and that consequently one of the possessors of this trait must have borrowed or come from the other. The second portion of Hrozný's observation, that the artistic representation of the fight between Gilgames and Enkidu with a bull represents the process of domestication of the bull by man, is equally open to objection. If this fight is a game and this game is considered a representation of man's mastery over the bull, then what would the Chinese cockfight and American football represent? The question is not fantastic or even ridiculous for, if we allow Hrozný to argue thusly about the cultural history in the ancient world, we must also allow some future student to use the same principles to understand the modern world.

It is this same lack of concern for rules of evidence that causes the author to make facile generalizations about migration from the region of Central Asia bordering upon the Caspian Sea, Altai, and Pamir. He declares (p. 236) that

The region was settled by nomads or semi-nomads who gradually succeeded in taming some animals and on the border of the steppe started the primitive agriculture which, in the course of time, developed into hoe-agriculture. The continuous drying up of these regions in the Post-glacial Age made the people migrate in different directions.

It is interesting to note that among the modern social scientists geographic determinism has long gone

out of fashion, for peoples do not move out of a given habitat simply because of a change of the physical environment even though the change may be for the worse. The fact is, some peoples moved out of Central Asia while others obviously remained to this day. The ones who moved out might even have done so when the place was drying up, and they might even have been somewhat conscious of it but judging by the fact that many others remained in spite of the geographic change, the theory of geographic determinism obviously needs serious modification or elimination.

Throughout, there is reference to various strains of the ancient civilization in the Near East as being of Oriental origin because they occurred in geographic Western Asia. One can almost detect a trace of an anxiety to demonstrate that the ancestral civilizations of the West were not wholly "Oriental," but that "the Indo-Europeans mightedly interfered with happenings in the Orient and to a great extent took part in building up its civilization" (p. viii). Had Hrozný looked into the functional and the psychological characteristics of the early civilizations (he and a majority of other scholars have described it as Oriental), he might have found that they are not Oriental at all but distinctly Occidental and unmistakably represent an ancestral variety of civilization more akin to the modern West than to the traditional East. Therefore, even without the "discovery" that a distinctly Indo-European people had interfered "mightedly" with the shaping of ancient civilization in the Near East, one could have pushed the prehistoric history of a distinctly Western civilization to a much earlier age.

The book is well printed and beautifully illustrated, but the binding is horrible. Even with very gentle handling while reading, the hard cover became separated from the pages.

FRANCIS L. K. HSU

Department of Anthropology, Northwestern University

Irrigated Soils. Their fertility and management. D. W. Thorne and H. B. Peterson. Blakiston, New York, ed. 2, 1954. xii + 392 pp. Illus. \$6.50.

THE authors are members of the staff of the Utah Agricultural College at Logan. Their training and experience make them familiar with alkaline and saline soils on which irrigation is largely practiced. They point out that 55 percent of the land area of the U.S. receives less than 20 in. of rainfall and that irrigation is a necessity if such land is to be cropped.

Of the 21 chapters in this edition, three are new (on soil and plant relations, planning a farm irrigation system, and planning the irrigated farm). The remaining chapters have been redeveloped by combining, deleting, and rewriting. As a result, the book has been markedly improved. It gives a very good idea of the nature of soil, water relationships, salt problems, and qualities of irrigation water; how to plan irrigation systems, apply water, reclaim alkali soils, control the physical, biological, and chemical properties of such soils; and how to manage irrigation for particular crops, including

grain, forage crops, fruits, vegetables, and specialty crops. The last chapter includes the making of a farm map, a soil map, a farm layout, an inventory, and a written report. The excellent illustrations, including diagrams and graphs, constitute a very important part of the book, as do the lists of references at the ends of the chapters. A glossary and some useful values for calculating purposes are appended. Although intended for use as a textbook in agricultural colleges in the semiarid and arid regions of the West, *Irrigated Soils* will also be highly useful to technicians and to intelligent farmers in such regions. It will also be of great interest and value to soil scientists the world over.

FIRMAN E. BEAR

Editor-in-chief, Soil Science, Rutgers University

Narcotics and Narcotic Addiction. David W. Maurer and Victor H. Vogel. Thomas, Springfield, Ill., 1954. 303 pp. Illus. \$7.50.

FOR many years the lay public and many professional workers in the medical, social, and law enforcement fields have been exposed to considerable quantities of nebulous nonsense concerning narcotic addiction. I know of no area in which so much inaccurate information has been accepted as gospel by the uninformed as well as by those who should know better. *Narcotics and Narcotic Addiction*, by Maurer and Vogel, gives a direct, accurate, and complete account of the problem and goes a long way in de-bunking the misinformation that has been disseminated. The approach to the diagnosis and treatment of narcotic addiction has often been made in either a temerous or hostile manner, but seldom with scientific objectivity. This book performs a great service in establishing the study of narcotic addiction on a scientific basis.

I wish to take exception with some of the statements made concerning etiological factors. In our experience with a large group of addicts under 21 years of age, we did not see any normal individuals who were inadvertently addicted because of group or community associations. It is true that many young boys and girls yielded to group pressure and experimented with narcotic drugs. However, if these adolescents were "normal" they abandoned the use of narcotic drugs after one or two trials because the drug had no meaning to them. Those who went on to addiction were addict-prone individuals who were unable to solve emotional, environmental, or cultural problems in any other way. They were aberrated individuals with destructive domestic, social, and community backgrounds. Many of the addicts were either schizophrenic or psychopathic, but none were normal. Their aberrated status was not secondary to the use of narcotic drugs, but their emotional abnormality made it possible for them to become addicted if exposed to drugs. We felt that in many instances the addiction was accidental because the individual happened to reside in a community where contraband narcotics were readily available. If these unfortunates had lived in a narcotic-free area, the symptomatology would have been alcoholism or some

other form of destructive social behavior, but they would not have become narcotic addicts. We were also of the opinion that the treatment of addiction itself was not too difficult in a controlled hospital setting if good medical disciplines were used. However, the diagnosis and treatment of the underlying personality disorder is extremely difficult and requires long term hospital care, mandatory follow-up and investigation of domestic situations on a psychiatric case work basis. Even if all these factors are thoroughly studied and even though treatment is extensive and prolonged, the prognosis in many cases is still guarded.

This is an excellent book and it should be in the possession of everyone who is interested in this problem and who works with narcotics and narcotic addiction. If the facts contained in this book were thoroughly assimilated, the problem of narcotic addiction could be approached with objective intelligence and the results would perhaps be more gratifying.

JEROME L. LEON

*JCRS Hospital, Spivak, Colorado;
Riverside Hospital for Adolescent Narcotic Addicts,
Bronx, New York*

Fluoridation as a Public Health Measure. James H. Shaw, Ed. AAAS, Washington 5, 1954. v+232 pp. Illus. \$4.50 (AAAS members, \$4).

SINCE 1941, the American Association for the Advancement of Science has published three monographs on the relationship of fluoride and dental caries. The most recent one, *Fluoridation as a Public Health Measure*, maintains the high standards set by its predecessors, *Fluorine and Dental Health*, appearing in 1941 and *Dental Caries and Fluorine*, published in 1944. The appearance of the monograph is especially timely because many American communities are presently considering the addition of fluorides to the water supply as a dental caries prevention measure. A well-organized opposition has questioned the effectiveness of the procedure, has claimed that such an undertaking is contraindicated because of the toxicity of fluorides, and has advocated the substitution of fluoride vehicles other than water.

The monograph refutes each of these suppositions. Stadt in the chapter on "Dental benefits of fluoride ingestion" concludes that

... the partial prevention of dental caries by means of water-borne fluorides is now a demonstrated fact.

Leone and his collaborators report a 10-year study of 116 people who drank water containing 8 ppm of fluoride. They were unable to show that such a regime "had produced detectable physiological effects." Similarly, Schlesinger and his collaborators summarize the 1942-52 findings for a group of more than 500 Newburgh, N. Y., children who ingested fluoride-supplemented water. They conclude that

... there has been no indication of any systemic effects, adverse or otherwise, from the use of fluoridated water on the basis of these findings.

Finally, Sognmaes, after giving careful consideration to the relative merits of various fluoridation vehicles, states that

... we as yet have little evidence except for water fluoridation with regard to the effect of fluoride supplement on caries in man. In view of this lack of specific knowledge, the use of any one fluoridated food item, other than water, whether liquid or solid, must be subjected to lengthy experimentation before it can be put to general use.

For those who are interested in the present status of topical applications of fluoride to the teeth as a dental decay preventive measure, the review by Bibby and Brudevold is highly recommended. The editor is to be commended for the over-all excellence of the publication.

J. F. VOLKER

University of Alabama School of Dentistry

Student Personnel Work as Deeper Teaching. Esther Lloyd-Jones and Margaret Ruth Smith, Eds. Harper, New York, 1954. xii + 361 pp. \$5.

THE student personnel program on our college campuses has taken shape and found many loyal adherents within the past 50 years. As such, it is peculiar to the individualization of mass education in the 20th century and has had to grow and expand through trial and error. The field has been fortunate, however, in having competent scholars who have continuously evaluated the growing process, criticizing some trends, lauding others. Two such educators and leaders are the editors of this book.

This is not, perhaps, a "first book" for a reader new to the field. It does, however, suggest a new approach to the general phase of organization and to a number of important facets found in programs of student personnel work. Herein is illustrated the means by which personnel service is endeavoring to integrate itself into the regular academic program.

This work has the weaknesses of most compilations in which a number of authors are involved (in this case, 26). The chapters vary in importance and worth.

Also, it deals somewhat vaguely with the general pattern of a decentralized program of personnel activities appropriate for greater faculty participation, or possibly in small colleges for faculty operation.

Such important specialties as the freshman program, vocational guidance, testing, personal and therapeutic psychological counseling, remedial and developmental reading programs, study programs, and English clinics are not included. In fact, the editors rather casually relegate most of these familiar services to specialists, on campus or off campus, to whom referral is suggested as the means of access. Well-qualified off-campus specialists in most of these areas are scarce and usually quite expensive. Esther Lloyd-Jones says:

The central Bureau and the central Department of Personnel Work must be seen in a different role.

... Its referral system needs expansion ... it should be facilitating communications among its communities and subgroups.

The editors do not make clear that in some situations this transformation is already in process.

A number of chapters are outstanding.

Mary Omer and Eugene Shepard of Stephens College have prepared the best chapter on personnel records and their functional use that I have seen.

Norman Kiell of Brooklyn College writes of the foreign student and his adjustment to American college life. This short essay, with its brief history of the foreign student in the U.S. and its suggestions regarding ways for meeting his needs, is both helpful and well written.

Anna Rose Hawkes, dean of Mills College, contributes a philosophic chapter on student discipline and suggests methods by which self-discipline can be instilled. She is the widow of the famous Dean Hawkes of Columbia College who was among the first to realize that when a student enters the classroom, he brings with him not only his mind but physical aches and mental disturbances. He comes as a "whole" student, as this volume so often reminds us.

William Hughes of Temple University stresses the importance of the physical education program. He points out that science has rejected the dichotomy of mind and body and that the ideal educational situation trains both mind and body.

Thomas Shrewsbury of City College, New York, provides a penetrating analysis of certain legal implications, important to student personnel workers.

Ordway Tead, professor of industrial personnel work at Columbia and editor at Harper Brothers, has contributed a chapter on "Developing spiritual insights" among students, an area on which he writes with more acumen than the usual author on religion or values.

College teachers, as well as professional personnel workers, will find this book a stimulating aid in understanding some more recent developments in the field of general personnel work. Despite its repetitious phrases and idealistic vocabulary, it provides a "new look" for much of student personnel work throughout the country. It indicates "a change of mind" on the part of the editors, which to me appears more as a developmental shift in emphasis than a change of mind. It is *must* reading for faculty teachers, for college administrators, and for professionals in personnel work.

LOUISE PRICE

Brooklyn College

Living Crafts. G. Bernard Hughes. Philosophical Library, New York, 1954. 192 pp. Illus. + plates. \$4.75.

A POPULAR historical account of 20 "ancient and honourable crafts that are practiced today solely because, for achieving certain results, no better methods have been found"—gold-beater, silversmith, textile printer, rope-maker, cooper, fireworks-maker, charcoal-burner, glass-blower, and so on. British methods are emphasized. An index is lacking.—D. R.

Books Reviewed in SCIENCE

10 December

- The Structures and Reactions of the Aromatic Compounds*, G. M. Badger (Cambridge Univ. Press.). Reviewed by R. L. Brown.
- Abnormal and Pathological Plant Growth*, Brookhaven National Laboratory (Office of Tech. Services, U. S. Dept. of Commerce). Reviewed by G. H. Starr.
- Elsevier's Encyclopaedia of Organic Chemistry*, ser. III: *Carboisocyclic compounds*, vol. 12B: *Naphthalene*, A. F. Radt, Ed. (Elsevier). Reviewed by P. A. S. Smith.
- Index IX to the Literature of American Economic Entomology*, Ina L. Hawes, compiler (Entomological Society of America). Reviewed by J. S. Wade.
- The Psychiatric Interview*, Harry Stack Sullivan; Helen Swick Perry and Mary Ladd Gawel, Eds. (Norton). Reviewed by C. K. Aldrich.
- How to Understand Propaganda*, Alfred McClung Lee (Rinehart). Reviewed by R. Braibanti.
- Atlas of Men*, William H. Sheldon (Harper). Reviewed by S. M. Garn.
- Anatomy of Weeds*, Emil Korsmo (Grondahl). Reviewed by W. H. Minshall.
- The Compleat Strategyst*, J. D. Williams (McGraw-Hill). Reviewed by O. Morgenstern.
- Linear Analysis*, Adriaan Cornelis Zaanen (North-Holland; Interscience). Reviewed by F. A. Ficken.
- The Statistical Approach to X-ray Structure Analysis*, Vladimir Vand and Ray Pepinsky (Pennsylvania State Univ.). Reviewed by D. Sayre.
- Mammalian Hybrids*, Annie P. Gray (Commonwealth Agricultural Bureaux, England). Reviewed by W. M. Mann.
- Autotrophic Micro-organisms*, B. A. Fry and J. L. Peel, Eds. (Cambridge Univ. Press). Reviewed by J. M. Smith.
- Elements of Food Engineering*, vol. II: *Unit Operations*, Milton E. Parker; Ellery H. Harvey and E. S. Stateker, Collabs. (Reinhold). Reviewed by A. G. Olsen.
- Heat Transmission*, ed. 3, William H. McAdams (McGraw-Hill). Reviewed by G. A. Hawkins.

17 December

- Administrative Medicine*, George S. Stevenson, Ed. (Josiah Macy, Jr. Foundation). Reviewed by D. D. Rutstein.
- Contributions to American Anthropology and History*, vol. XI (Carnegie Institution of Washington). Reviewed by R. F. Herzfeld.
- Theory of Functions of a Complex Variable*, vol. I, C. Carathéodory, trans. by F. Steinhardt (Chelsea Pub.). Reviewed by M. Heins.
- Wave Motion and Vibration Theory*, vol. V, Albert E. Heins, Ed. (McGraw-Hill). Reviewed by S. I. Pai.
- Fatigue of Metals*, ed. 3, of *La Fatigue des Metaux*, R. Cazaud, trans. by A. J. Fenner (Philosophical Library). Reviewed by R. R. Moore.
- Statistical Analysis in Chemistry and the Chemical Industry*, Carl A. Bennett and Norman L. Franklin (Wiley; Chapman & Hall). Reviewed by W. H. Clatworthy.
- Physical Geology*, L. Don Leet and Sheldon Judson (Prentice-Hall). Reviewed by H. Taylor.

- Beyond the Germ Theory*, Iago Galdston, Ed. (N.Y. Health Education Council). Reviewed by W. M. Johnson.
- The World of Learning 1954* (Europa Pub.). Staff review.

24 December

- Petroleum Microbiology*, Ernest Beerstecher, Jr. (Elsevier). Reviewed by C. E. ZoBell.
- Rare Metals Handbook*, Clifford A. Hampel, Ed. (Reinhold). Reviewed by M. G. Mellon.
- Theory of Games and Statistical Decisions*, David Blackwell and M. A. Girshick (Wiley; Chapman & Hall). Reviewed by S. Karlin.
- Physical Properties of Solid Materials*, C. Zwicker (Interscience; Pergamon). Reviewed by C. Wert.
- Elementary Introduction to Molecular Spectra*, Borge Bak (North-Holland; Interscience). Reviewed by J. R. Nielsen.
- Geschichte der Mathematik*, J. E. Hofmann (Walter de Gruyter). Reviewed by P. Rabinowitz.
- Précis de Minéralogie*, P. Lapadu-Hargues (Masson). Reviewed by H. T. Evans, Jr.
- Guide for Safety in the Chemical Laboratory*, General Safety Committee of the Manufacturing Chemists' Association (Van Nostrand). Reviewed by M. L. Perlman.
- An Introduction to Climate*, Glenn T. Trewartha, ed. 3, (McGraw-Hill). Reviewed by C. W. Thornthwaite.
- Directory of Hydrobiological Laboratories and Personnel in North America*, Robert W. Hiatt, Ed. (Univ. of Hawaii Press). Staff review.

31 December

- Textbook of Electrochemistry*, vols. I and II, G. Kortüm and J. O'M. Bockris (Elsevier). Reviewed by G. E. Boyd.
- Introduction to Opinion and Attitude Measurement*, H. H. Remmers (Harper). Reviewed by W. D. Rugg.
- Ion Transport across Membranes*, Hans T. Clarke, Ed., David Nachmansohn, Assoc. Ed. (Academic Press). Reviewed by L. J. Mullins.
- Histopathologic Technic and Practical Histochemistry*, rev. ed. of *Histopathologic Technic*, R. D. Lillie (Blakiston). Reviewed by H. S. Bennett.
- Tables Numériques de Physique Nucléaire*, Charles Noël Martin (Gauthier-Villars). Reviewed by E. Segrè.
- Organic Chemistry*, Lawrence H. Amundsen (Holt). Reviewed by Frances Berliner.
- Residual Stresses in Metals and Metal Construction*, William R. Osgood, Ed. (Reinhold). Reviewed by R. A. Kelsey.
- Molecular Theory of Gases and Liquids*, Joseph O. Hirschfelder, Charles F. Curtiss, and R. Byron Bird (Wiley; Chapman & Hall). Reviewed by D. ter Haar.
- Peripheral Circulation in Man*, G. E. W. Wolstenholme and Jessie S. Freeman, Eds. (Little, Brown). Reviewed by F. P. Ferguson.
- A Curriculum for Schools of Medical Technology*, ed. 3, Israel Davidsohn, Ed. (Registry of Medical Technologists, American Society of Clinical Pathologists). Staff review.

New Books

- Quantum Mechanics.** F. Mandl. Academic Press, New York-Butterworths, London, 1954. viii + 233 pp. Illus. \$5.80.
- Proceedings of the Symposium on Operations Research in Business and Industry.** Midwest Research Inst., Kansas City, Mo., 1954. iv + 185 pp. Illus. Paper, \$5.
- The Fundamentals of Electric Log Interpretation.** M. R. J. Wyllie. Academic Press, New York, 1954. x + 126 pp. Illus. \$3.60.
- Formation des Continents et Progression de la Vie.** H. Termier and G. Termier. Masson, Paris, 1954. 135 pp. Illus. + plates. Paper, F. 750.
- Les Amphorismes Ypocras de Martin de Saint-Gille, 1362-1365.** G. Lafeuille. Harvard Studies in Romance Languages, vol. XXV. Harvard Univ. Press, Cambridge, 1954. 165 pp. Plates. \$5.50.
- Factors Affecting the Costs of Hospital Care.** vol. 1 of *Financing Hospital Care in the United States*. John H. Hayes, Ed. Blakiston, New York, 1954. xvii + 300 pp. \$4.
- The Identification of Organic Compounds.** A manual of qualitative and quantitative methods. Stig Veibel. Gad, Copenhagen, ed. 4 (Eng. ed. 1), 1954. xv + 346 pp. Illus.
- Modern Experiments in Telepathy.** S. G. Soal and F. Bateman. Yale Univ. Press, New Haven, 1954. xv + 425 pp. Illus. + plate. \$5.
- Minerals for Atomic Energy.** A guide to exploration for uranium, thorium, and beryllium. Robert D. Nininger. Van Nostrand, New York-London, 1954. xii + 367 pp. Illus. + plates. \$7.50.
- The Manual of Antibiotics, 1954-1955.** Henry Welch, Ed. Medical Encyclopedia, New York, 1954 (Order from American Pharmaceutical Assoc., 2215 Constitution Ave., NW, Washington, D.C.). 87 pp. \$2.50.
- Liver Injury.** Trans. of the 12th conference, 21-23 Sept. 1953. F. W. Hoffbauer, Ed. Josiah Macy, Jr. Foundation, New York, 1954. 231 pp. Illus. + plate. \$4.25.
- Dairy Cattle Feeding and Management.** H. O. Henderson and Paul M. Reaves. Wiley, New York, ed. 4, 1954. 614 pp. Illus. \$6.50.
- An Introduction to Molluscan Ecology.** Distribution and population studies of fresh-water molluscs. Alan Mozley. Lewis, London, 1954. x + 71 pp. Illus. 9s.
- Introduction to Atomic and Nuclear Physics.** Henry Semat. Rinehart, New York, ed. 3, 1954. xii + 561 pp. Illus. \$6.50.
- The Distribution and Abundance of Animals.** H. G. Andrewartha and L. C. Birch. Univ. of Chicago Press, Chicago, 1954. xv + 782 pp. Illus. \$15.
- General Zoology Laboratory Study Guide.** Sumner O. Burhoe. Burgess, Minneapolis, rev. ed., 1954. vi + 120 pp. Illus. Paper, \$2.40.
- Cherries and Cherry Products.** Roy E. Marshall. vol. V of *Economic Crops*; Z. I. Kertesz, Ed. Interscience, New York-London, 1954. xiv + 263 pp. Illus. \$8.50.
- Business Management.** Efficiency surveys and systems. John A. Shubin. Barnes and Noble, New York, 1954. x + 372 pp. Illus. Paper, \$1.75.
- General Chemistry.** W. Norton Jones, Jr. Blakiston, New York, 1954. xii + 907 pp. Illus. \$6.50.
- Recent Progress in Hormone Research.** vol. X. Proc. of the Laurentian Hormone Conference. Gregory Pin-cus, Ed. Academic Press, New York, 1954. 511 pp. Illus. \$9.80.
- Sir William Petty.** Portrait of a genius. E. Strauss. Free Press, Glencoe, Ill., 1954. 260 pp. \$5.
- The Sun, the Sea, and Tomorrow.** Potential sources of food, energy and minerals from the sea. F. G. Walton Smith and Henry Chapin. Scribner, New York, 1954. xii + 210 pp. Illus. \$3.50.
- Micro and Semimicro Methods.** Nicholas D. Cheronis. vol. VI of *Technique of Organic Chemistry*; Arnold Weissberger, Ed. Interscience, New York-London, 1954. xxiii + 628 pp. Illus. \$12.
- Meredith's Hygiene.** A textbook for college students on physical, mental, and social health from personal and public aspects. Arthur F. Davis and Warren H. Southworth. Blakiston, New York-Toronto, ed. 5, 1954. xv + 906 pp. Illus. \$6.
- Introduction to Modern Algebra and Matrix Theory.** Ross A. Beaumont and Richard W. Ball. Rinehart, New York, 1954. xii + 331 pp. \$6.
- Directory of International Scientific Organizations.** UNESCO, Paris, ed. 2, 1954 (Distr. by Columbia Univ. Press, New York). 312 pp. Paper, \$2.50.
- Directory of Hydrobiological Laboratories and Personnel in North America.** Robert W. Hiatt, Ed. Univ. of Hawaii Press, Honolulu, 1954. ix + 324 pp. Illus. \$3.75.
- An Outline of Developmental Physiology.** C. P. Raven. Trans. by L. de Ruiter. McGraw-Hill, New York and Pergamon Press, London, Engl. ed. 1, 1954. viii + 216 pp. Illus. + plates. \$5.50.
- An Introduction to Human Biochemical Genetics.** H. Harris. Eugenics Laboratory Memoirs, XXXVII. Cambridge Univ. Press, New York, 1953. 96 pp. Illus. \$2.75.
- Flash!** Seeing the unseen by ultra high-speed photography. Harold E. Edgerton and James R. Killian, Jr. Branford, Boston, ed. 2, 1954. 215 pp. Illus. \$6.50.
- Galen of Pergamon.** George Sarton. Univ. of Kansas Press, Lawrence, 1954. 112 pp. Illus. \$2.50.
- Animal Form and Function.** An introduction to college zoology. W. R. Breneman. Ginn, Boston, 1954. viii + 488 pp. Illus. \$6.
- Animal Cytology and Evolution.** M. J. D. White. Cambridge Univ. Press, New York, ed. 2, 1954. xiv + 454 pp. Illus. \$8.50.
- My Life with the Microbes.** Selman A. Waksman. Simon & Schuster, New York, 1954. xii + 364 pp. \$5.
- America's Resources of Specialized Talent.** A current appraisal and a look ahead. Report of the Commission on Human Resources and Advanced Training; Dael Wolfe, director. Harper, New York, 1954. xviii + 332 pp. Illus. \$4.
- Human Heredity.** James V. Neel and William J. Schull. Univ. of Chicago Press, Chicago, 1954. vii + 361 pp. Illus. \$6.
- Effects of Electricity on Muscular Motion.** Luigi Galvani. Trans. by Margaret Glover Foley. Burndy Library. Norwalk, Conn., 1954. 176 pp. Illus. + plates. \$6.
- Freaks and Marrels of Insect Life.** Harold Bastin. Wyn, New York, 1954. 248 pp. Illus. + plates. \$3.75.

- Science Milestones.** The story of the epic scientific achievements and the men who made them possible. George B. Clementson, Ed. Windsor Press, Chicago, 1954. 312 pp. Illus. \$5.
- The Psychology of Invention in the Mathematical Field.** Jacques Hadamard. Dover, New York, 1954. Reprint of ed. 1 (1945). xiii + 145 pp. Paper, \$1.25; cloth, \$2.50.
- Principles of Internal Medicine.** T. R. Harrison, Raymond D. Adams, William H. Resnik, Paul B. Beeson, George W. Thorn, and M. M. Wintrobe, Eds. Blakiston, New York-Toronto, ed. 2, 1954. xxiii + 1703 pp. Illus. + plates. Student, 1-vol. ed., \$16.; professional, 2-vol. ed. boxed, \$21.
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- A Treatise on Electricity and Magnetism.** vols. I and II. James Clerk Maxwell. Dover, New York, 1954. An unabridged reprint of the last rev. ed. 3 (1891). vol. I, xxxii + 506 pp.; vol. II, xxiv + 500 pp. Illus. + plates. \$4.95.
- The Technology of Solvents and Plasticizers.** Arthur K. Doolittle. Wiley, New York; Chapman & Hall, London, 1954. xv + 1056 pp. Illus. \$18.50.
- The Study of the Brain.** A companion text to the stereoscopic atlas of neuroanatomy. Hyman S. Rubinstein. Grune and Stratton, New York, 1953. xiii + 209 pp. Illus. + plates. \$9.50.
- Streams, Lakes, Ponds.** Robert E. Coker. Univ. of North Carolina Press, Chapel Hill, 1954. xviii + 327 pp. Illus. + plates. \$6.
- The Story of Man.** From the first human to primitive culture and beyond. Carleton S. Coon. Knopf, New York, 1954. xxii + 437 pp. Illus. + plates. \$6.75.
- Packaging Engineering.** Louis C. Barail. Reinhold, New York, 1954. vii + 407 pp. Illus. \$9.50.
- Oral Pathology.** A histological, roentgenological, and clinical study of the diseases of the teeth, jaws, and mouth. Kurt H. Thoma. Mosby, St. Louis, ed. 4, 1954. xviii + 1536 pp. Illus. + plates. \$22.50.
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- A Guide to the Planets.** Patrick Moore. Norton, New York, 1954. 254 pp. Illus. + plates. \$4.95.
- Chemistry of Lichen Substances.** Yasuhiko Asahina and Shoji Shibata. Japan Society for the Promotion of Science, Tokyo, 1954 (Order from Maruzen Co., Nihonbashi, Tokyo). 240 pp. \$2.50.
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- Organic Analysis.** vol. II. John Mitchell, Jr., I. M. Kolthoff, E. S. Proskauer, and A. Weissberger, Eds. Interscience, New York-London, 1954. 372 pp. \$8.50.
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- Science Reasoning and Understanding.** A handbook for college teachers. Intercollege Committee on the Evaluation of Science Objectives of the Cooperative Study of Evaluation in General Education, Paul L. Dressel, director. Brown, Dubuque, Iowa, 1954. 223 pp. Paper, \$3.50.
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- General Theory of High Speed Aerodynamics.** vol. VI of *High Speed Aerodynamics and Jet Propulsion*. W. R. Sears, Ed. Princeton Univ. Press, Princeton, 1954. 758 pp. \$15.
- The Art of Making Sense.** A guide to logical thinking. Lionel Ruby. Lippincott, Philadelphia, 1954. 286 pp. \$3.75.
- Medicina Nucleare.** Introduzione alle applicazioni medico-biologiche della fisica nucleare. Aldo Perussia, U. Facchini, E. Gatti, L. Malatesta, C. Salvetti, and M. Silvestri. Pensiero Scientifico, Rome, 1954. 877 pp. L. 7000.
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(Continued on page xi)

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- Lehrbuch der Palaobotanik.** Walther Gothan and Hermann Weyland. Akademie, Berlin, 1954. 535 pp. DM. 46.
- Volume Jubilaire, Victor Van Straelen, directeur de l'Institut Royal des Sciences Naturelles de Belgique, 1925-1954.** vols. I and II. Institut Royal des Sciences Naturelles de Belgique, Brussels, 1954. 1213 pp.
- Time Distortion in Hypnosis.** An experimental and clinical investigation. Lynn F. Cooper and Milton H. Erickson. Williams & Wilkins, Baltimore, 1954. 191 pp. \$4.
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- A History of Birds.** James Fisher. Houghton Mifflin, Boston, 1954 (first U.S. publ.). 205 pp. \$3.75.
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- Elements of Algebra.** Howard Levi. Chelsea, New York, 1954. 160 pp. \$3.25.
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- Exercises to Higher Algebra.** Helmut Hasse and Walter Klobe. Trans. by Theodore J. Benac from rev. German ed. 2. Ungar, New York, 1954. 212 pp. \$4.
- A Budget of Paradoxes.** vols. I and II. Augustus de Morgan. Dover, New York, ed. 2, 1954. Unabridged republication of ed. 2 (1915). 789 pp. \$4.95.

- Sex and Morality.** Abram Kardiner. Bobbs-Merrill, New York, 1954. 266 pp. \$3.
- Advanced Organic Chemistry.** E. Earl Royals. Prentice-Hall, New York, 1954. 948 pp. \$12.
- The Third Revolution.** A study of psychiatry and religion. Karl Stern. Harcourt, Brace, New York, 1954. 306 pp. \$4.
- Fluoridation as a Public Health Measure.** James H. Shaw, Ed. AAAS, Washington 5, 1954. 232 pp. \$4.50; AAAS members, \$4.
- Textbook of Physics.** R. Kronig, Ed. In collaboration with J. de Boer, H. C. Burger, P. H. Van Cittert, C. J. Gorter, A. C. S. Van Heel, P. Van der Leeden, and G. J. Sizoo. Interscience, New York; Pergamon, London, 1954. 855 pp. \$10.
- Chemical Specificity in Biological Interactions.** Harvard Mem. No. 3, Frank R. N. Gurd, Ed. Academic Press, New York, 1954. 234 pp. \$6.
- Life on Other Worlds.** Harold Spencer Jones. English Universities Press, London, rev. ed. 2, 1954 (Distributed by Macmillan, New York 11) 259 pp. \$3.
- The Language of Taxonomy.** An application of symbolic logic to the study of classificatory systems. John R. Gregg. Columbia Univ. Press, New York 27, 1954. 70 pp. \$2.50.
- Elements of Ecology.** George L. Clarke. Wiley, New York; Chapman & Hall, London, 1954. 534 pp. \$7.50.
- Pharmacology in Medicine.** A collaborative textbook. Victor A. Drill, Ed. McGraw-Hill, New York-London, 1954. 87 chapters \$19.50.
- Hugh Roy Cullen: A Story of American Opportunity.** Ed Kilman and Theon Wright. Prentice-Hall, New York, 1954. 376 pp. \$4.
- Science Funbook.** Gerald M. Straight. Hart, New York, 1954 (For ages 10-15). 159 pp. Paper, \$1.25.
- The Genetics of *Paramecium aurelia*.** Monographs in Experimental Biology, 2. G. H. Beale. Cambridge Univ. Press, New York 22, 1954. 179 pp. \$2.50.
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- Cancer: New Approaches, New Hope.** Boris Sokoloff. Devin-Adair, New York, 1952. 243 pp. \$3.75.
- Fruits, Vegetables, and Flowers.** Physiology and structure in relation to economic use and market quality. George M. Kessler. Burgess, Minneapolis, 1954. 67 pp. Paper, \$2.
- Manufacture and Application of Lubricating Greases.** C. J. Boner. Reinhold, New York 22, 1954. 977 pp. \$18.50.
- The North American Prairie.** John E. Weaver. Johnsen, Lincoln, Neb., 1954. 348 pp. \$5.
- Progress in the Chemistry of Organic Natural Products.** vol. 11 (in English and German). L. Zechmeister, Ed.: A. Albert, K. Bruckner, R. B. Corey, K. Freudenberg, H. H. Inhoffen, R. Lemberg, L. Pauling, S. Peat, H. Schmid, W. A. Schroeder. Springer, Vienna, 1954. 457 pp. Paper, \$17.20; cloth, \$18.
- Deterioration of Materials.** Causes and preventive techniques. Glenn A. Greathouse and Carl J. Wessel, Eds. Reinhold, New York, 1954. 835 pp. \$12.
- History of the Primates.** An introduction to the study of fossil man. W. E. le Gros Clark. British Museum (Natural History), London, ed. 4, 1954. 119 pp. Paper, 4s.

- Asia, East by South.** A cultural geography. Joseph E. Spencer. Wiley, New York; Chapman & Hall, London, 1954. 453 pp. \$8.50.
- Der Scharlach und Seine Behandlung.** W. Pulver. Huber, Bern-Stuttgart, 1954. 209 pp. Paper, DM. 19.80.
- The Scourge of the Swastika.** A short history of Nazi war crimes. Lord Russell. Philosophical Library, New York, 1954. 259 pp. \$4.50.
- Isotopic Gas Analysis for Biochemists.** R. F. Glascock. Academic Press, New York 10, 1954. 247 pp. \$5.80.
- A Field Guide to Animal Tracks.** Olaus J. Murie. Peterson Field Guide Series, No. 9. Roger Tory Peterson, Ed. Houghton Mifflin, Boston, 1954. 374 pp. \$3.75.
- Renal Function.** Trans. of the Fifth Conference 14-16 Oct. 1953, Princeton, N.J. Stanley E. Bradley, Ed. Josiah Macy, Jr., Fdn., New York, 1954. 218 pp. \$3.75.
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- The Nation Looks at Its Resources.** Report of the Mid-Century Conference on Resources for the Future. 2-4 Dec., 1953, Washington. Resources for the Future, Inc., Washington, 1954. 418 pp. Paper, \$5.
- Ordovician Cephalopod Fauna of Baffin Island.** Mem. 62. A. K. Miller, Walter Youngquist, and Charles Collinson. Geological Society of America, New York 27, 1954. 234 pp.
- Climatic Atlas of the United States.** Stephen Sargent Visser. Harvard Univ. Press, Cambridge, 1954. 403 pp. \$9.
- Public Education under Criticism.** C. Winfield Scott and Clyde M. Hill, Eds. Prentice-Hall, New York, 1954. 414 pp. \$6.35.
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- Prehistoric Stone Implements of Northeastern Arizona.** Richard B. Woodbury. vol. XXXIV, Rpt. No. 6. Peabody Museum of American Archaeology and Ethnology, Cambridge, Mass., 1954. 240 pp. Paper, \$7.50.
- Transactions of the International Astronomical Union.** vol. VIII. Eight General Assembly, 4-13 Sept. 1952, Rome. P. Th. Oosterhoff, Ed. Cambridge Univ. Press, New York 22, 1954. 887 pp. \$10.50.
- The Art of Primitive Peoples.** J. T. Hooper and C. A. Burland. Philosophical Library, New York 16, 1954. 168 pp. \$7.50.
- Pharmakotherapie des Fiebers und der fieberhaften Affektionen.** R. Isenschmid, E. Glanzmann, H. Berger, and T. Gordonoff. Huber, Bern-Stuttgart, 1954. 360 pp. DM. 29.80.
- Physical Methods of Organic Chemistry.** vol. I, pt. III of *Technique of Organic Chemistry*. Arnold Weissberger, Ed. Interscience, New York-London, rev. ed. 2, 1954. 433 pp. \$8.50.
- Structure of Molecules and Internal Rotation.** San-ichiro Mizushima. Physical Chemistry, a series of monographs. Eric Hutchinson, Ed. Academic Press, New York 10, 1954. 244 pp. \$6.
- Handbook of Textile Fibers.** Milton Harris, Ed. Harris Research Laboratories, Washington 11, 1954. 356 pp. \$12.50.
- Table of the Gamma Function for Complex Arguments.** Applied Mathematics ser. 34. Natl. Bur. of Standards, Washington 25, 1954 (Order from Supt. of Documents, GPO, Washington 25). 105 pp. \$2.
- Song of the Sky.** Guy Murchie. Houghton Mifflin, Boston, 1954. 438 pp. \$5.

Meetings

February

- 17-19. American Acad. of Forensic Sciences, Los Angeles, Calif. (W. J. R. Camp, 1853 Polk St., Chicago 12, Ill.)
- 28-1. American Orthopsychiatric Assoc., 32nd annual, Chicago, Ill. (Marion F. Langer, AOA, 1790 Broadway, New York 19.)
- 28-2. American Educational Research Assoc., St. Louis, Mo. (F. W. Hubbard, 1201 16 St., NW, Washington 6, D.C.)

March

- 2-4. American Assoc. of University Professors, Gatlinburg, Tenn. (R. E. Himstead, AAUP, 1785 Massachusetts Ave., NW, Washington 6, D.C.)
- 7-9. Chemical Inst. of Canada, Divisional Conf. of the Chemical Engineering Div., Ottawa, Ont. (W. M. Campbell, Box 323, Deep River, Ont.)
- 7-11. American Soc. of Photogrammetry, Washington, D.C. (C. E. Palmer, 1000 11 St., NW, Washington 1.)
- 7-11. National Assoc. of Corrosion Engineers, 11th annual, Chicago, Ill. (A. B. Campbell, 1061 M & M Bldg., Houston 2, Tex.)
- 9-11. American Cong. on Surveying and Mapping, 15th annual, Washington, D.C. (Walter S. Dix, Room 435, Woodward Bldg., Washington 5.)
- 10-12. M. D. Anderson Symposium on Fundamental Cancer Research, Houston, Tex. (Univ. of Texas, M. D. Anderson Hospital and Tumor Inst., Houston.)
14. American Educational Research Assoc., Denver, Colo. (F. W. Hubbard, 1201, 16 St., NW, Washington 6, D.C.)
14. Wildlife Soc., Montreal, Canada. (D. L. Leedy, Fish and Wildlife Service, Washington 25, D.C.)
- 15-17. Electrical Utilization of Aluminum, American Inst. of Electrical Engineers, Pittsburgh, Pa. (N. S. Hibshem, AIEE, 33 W. 39 St., New York 18.)
- 17-19. American Physical Soc., Baltimore, Md. (K. K. Darrow, Columbia Univ., New York 27.)
- 17-19. International Symposium on Cardiovascular Surgery, Detroit, Mich. (John Keyes, Henry Ford Hospital, Detroit 2.)
- 17-19. National Wildlife Federation, Montreal, Canada. (C. H. Callison, 232 Carroll St., NW, Washington 12, D.C.)
- 17-2. Inter-American Statistical Conf., 3rd, Santiago, Chile. (IASI, Pan American Union, Washington 6, D.C.)
- 20-23. American Assoc. of Dental Schools, annual, Chicago, Ill. (M. W. McCrea, 42 S. Greene St., Baltimore 1, Md.)
- 21-23. Acro Medical Assoc., 26th annual, Washington, D.C. (R. J. Benford, P. O. Box 1607, Washington 13.)
- 24-26. National Science Teachers Assoc., Cincinnati, Ohio. (R. H. Carleton, 1201 16 St., NW, Washington, D.C.)
- 28-31. American Assoc. of Petroleum Geologists, New York, N.Y. (E. H. Powers, Box 670, Fort Worth, Tex.)
- 28-1. Western Metal Exposition, 9th, Los Angeles, Calif. (W. H. Eisenman, 7301 Euclid Ave., Cleveland 3, Ohio.)
- 31-2. Soc. of Research in Child Development, Monticello, Ill. (C. B. Stendler, College of Education, University of Illinois, Urbana.)